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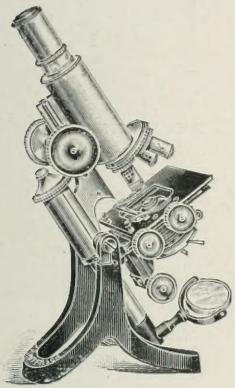

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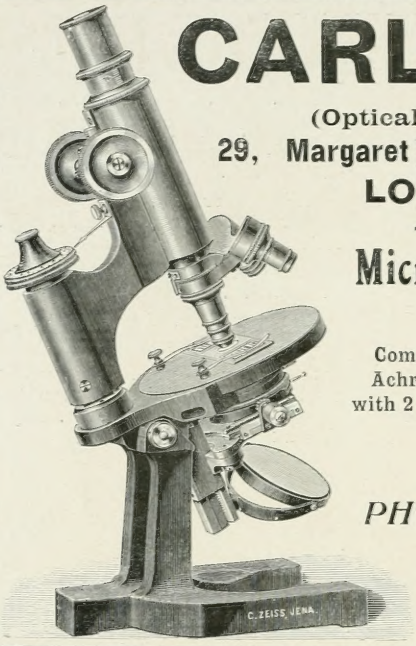

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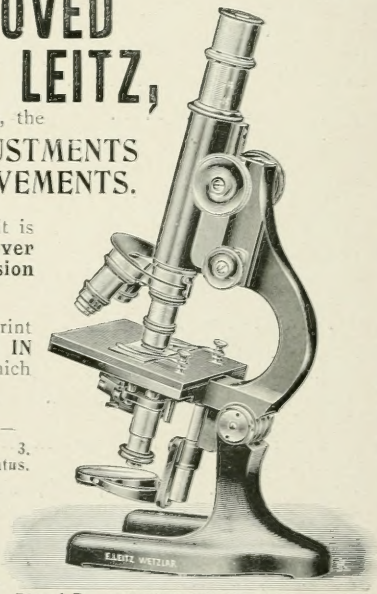

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ERRATA.

THE SEQUENCE OF PLANT REMAINS IN THE BRITISH PEAT MOSSES.

Owing to the author's absence abroad, and the proofs not reaching him until after publication, several misprints occur in the text.

The following are the chief errata :—

Page 318, line 32, for *plot*, read *foot*.

,, 320, ,, 21, ,, *Bycopus*, read *Lycopus*.

,, 321, ,, 12, ,, *Stellaria media*, read *Chrysanthemum Segetum*.

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On the death of Lord Kelvin there passed away one who will always remain identified with the remarkable progress achieved by science during the nineteenth century. At the celebration, in June 1896, in honour of the jubilee of his professorship at Glasgow, Lord Kelvin referred to his original contributions to science "as a record of persevering endeavour to see below the surface of matter." This endeavour resulted in profoundly modifying some of the conditions of life for many of the inhabitants of this world, in virtue of his extraordinary faculty of applying to practical matters the fruits of his researches. In

this connection it is not necessary to do more than refer to the great part he played in the development of submarine telegraphy, in the improvements he effected in the sounding plummet and the mariner's compass, in the invention of accurate and sensitive instruments for electrical measurements, and in the general advancement of electrical engineering. His researches on the tides added greatly to our knowledge, both theoretical and practical, of these important phenomena, and furnished another line of attack on the problem of solving the age of this earth.

In pure science, by his researches in thermodynamics, wave motion and vortex motion, Lord Kelvin advanced greatly human knowledge of the properties of matter. The whole civilised world recognised him as one of the most brilliant natural philosophers of all time, and now mourns his loss.

THE PHOTOGRAPHY OF COLOUR

By CHAPMAN JONES, F.I.C., F.C.S., ETC.

Royal College of Science, London

I

As this problem has engaged the attention of innumerable workers for nearly a hundred years, and the best methods so far known are now carried out on a considerable scale, it is obviously impossible to deal with the subject with any approach to completeness within the compass of an article such as this. I propose, therefore, to consider the principles involved rather than the practical details, and to deal with the subject broadly rather than minutely.

It is worth while first to ask, What is meant by the expression "photography of colour"? There is no doubt whatever as to what it means in its simplest conception. Ordinary photography depicts form with an accuracy and minuteness that far exceeds the best handwork; and by special care—that is, the use of special lenses and of cameras made after the manner of surveying instruments—it is possible to get results that may be depended upon for surveying purposes. Not only is correct "drawing" inherent in photography, but also the rendering of light and shade—that is, the comparative luminosities of the various parts of the object, within certain limits that are well understood. The simplest idea and the most acceptable method of the photography of colour is that in which the colour of the object is reproduced as automatically and correctly as its shape, and shall be visible on ordinary inspection as the colours of a painting are. Such colour photography has been eagerly sought for but is as yet impossible; and if one is justified in judging of the future in the light of the past, it will remain impossible. There does not seem any line of work that can lead to such a result. From this point of view colour photography as now practised is always a compromise.

The methods of both the past and the present naturally

fall into two great classes, or, more correctly perhaps, a greater class and a lesser class. The less important division includes methods in which a single homogeneous surface is employed, while in the larger division the surface is multiple or non-homogeneous. The first is generally an attempt to get as near as possible to the simple colour photography described above, while the second is entirely indirect in that it does not aim at producing colour at all, but only at automatically locating suitable dyes or inks. We will consider the direct method first.

Dr. T. J. Seebeck, of Jena, in the year 1810 or thereabouts, while making some experiments as to the action of the solar spectrum on silver chloride, observed that the violet light of the spectrum produced a reddish brown substance sometimes inclining towards violet, that the blue light produced a blue substance, and the red a rose or lilac substance. If the silver salt had previously been exposed to light for a time, so as to darken it, there was a bleaching action where the red and yellow light fell upon it. It is hardly likely that he had in mind any photographic process whether in colour or otherwise, as photography at this time was not a practical art. Eight years before this Sir Humphry Davy had described at the Royal Institution the attempts of Thomas Wedgwood, which he had supplemented, to get a practical photographic method—a printing process as we should now call it—but their preparations were very lacking in sensitiveness, and no method of fixing them was known. Of those who were primarily concerned in working out practical photographic methods, the elder Niepce began his work in 1814, Daguerre in 1824, and Fox Talbot in 1833, and it was not until 1839 that their work culminated in serviceable processes.

Edmund Becquerel appears to have been the first to specifically attack the problem of the photography of colour. It is stated that he began his investigations in 1838, though he did not publish any results of them until ten years later. Although the earlier date was just before the methods of Daguerre and Talbot were published, there is no doubt that there was a general knowledge in scientific circles that something of the kind would shortly be made known, and that this directed the attention of many investigators to the possibilities of photography. But long before 1848 other workers had made observations concerning the photography of colour. Sir John

Herschel had repeated Seebeck's experiment; Daguerre and Fox Talbot had both observed that in some of their photographs a red object produced a red colour in the picture; and Robert Hunt had succeeded in obtaining colours on paper impregnated with various silver salts. Hunt put, for example, a prepared sheet under red, yellow, green, and blue glasses, and exposed the arrangement for a week to diffused light. The silver chloride "became red under the red glass, dirty yellow under the yellow glass, a dark green under the green, and a light olive under the blue." But with a sheet "differently prepared" he tells us he obtained "a very beautiful picture," in which the sky was crimson, stucco-fronted houses came out slaty blue, and the green fields of a brick-red tint.

Becquerel used silver plates and produced a layer of silver chloride on the surface of the metal, at first by the action of cupric chloride, but afterwards by the electrolysis of dilute hydrochloric acid with the silver plate as the positive electrode. The plates were treated until the very thin layer of the accumulating chloride on their surfaces had passed through certain definite colour-changes, and then washed, dried, and warmed. With these plates he reproduced, with a considerable measure of success, the colours of brightly dressed dolls and highly coloured designs besides the solar and electric arc spectra.

There were many others who busied themselves with experiments of this character at this time and for many years afterwards, making little variations and meeting with more or less success, sometimes propounding strange theories and endeavouring to justify them, and now and again expressing their hopes and almost their convictions that what they sought for was just coming within their reach. But their hopes were ill founded, for the work was purely empirical; they had no sound principles to guide them, and no real progress was ever made. Even the colours obtained were not permanent, and though some when considerable care was taken to preserve them lasted longer than others, the difficulty of "fixing" them, or rendering them even moderately stable, was never overcome. We shall shortly see something of the nature of the colours produced in these experiments.

If light waves are similar to the waves produced by disturbing the surface of water, as they are supposed to be, it follows that if a continuous series of waves is reflected back

upon its original path, the waves travelling in opposite directions will interfere with each other. It is clear that as they move in opposite directions each wave will alternately coincide with and oppose a wave of the series that it is meeting, and that where they coincide their moving power on the particle concerned will be added, and that where they are opposed the particle can obey neither impulse, and therefore remains stationary. There is thus produced a series of "standing waves," for although their crests rise and fall or move to and fro at (practically) right angles to the path of the disturbance, they move in neither direction along the path. The alternate crests move in opposite directions, and between each and the next there is a stationary point. If these conditions are realised in light waves, the light will be quenched at those points where the movement of the particles (of the luminiferous ether) is prevented, and there will result a series of layers of light with intervals of darkness half a wave length apart.

Dr. W. Zenker, of Berlin, in 1868 published a small volume, *Lehrbuch der Photochromie* (*Photographie in natürlichen Farben*), in which he examined in detail the methods, theories, and results of the work that had been done up to then in connection with the reproduction of colour. While satisfied that coloured light will under certain conditions produce something that reflects the same, or very nearly the same, colour as the incident light, he points out the insufficiency of the theories of the change that had been put forward. The colour of the incident light, though reproduced more or less in all cases, is especially noticeable when silver plates are used, as in the method of Becquerel. He then explains the nature of standing waves, and advances this as an explanation of the phenomenon. The silver chloride, especially if violet (that is, slightly reduced), is so affected by the light that metallic silver is produced in layers, with intervals of no chemical change which correspond to the parts where the light is quenched by interference. He supposed metallic silver to be produced because it is necessary for the reproduction of the colour that the product of the change shall be a good reflector. As to why reflecting laminæ of this kind should reflect only or chiefly light of the same wave length (or colour) as the light which produced the laminæ when they are illuminated by white light, he explains that light of a greater or less wave length than that of the original incident ray will be out

of harmony with the reflecting laminæ, so that the reflections from the various laminæ will be in dissimilar phases, and the resulting interference will cause this light to be more or less extinguished. But that part of the white light that corresponds in wave length to the original ray will be reflected at each lamina in such a manner that the difference in progression will always be equal to exactly a whole wave length, and therefore the light reflected from each surface will reinforce that from every other. He considered that this explanation followed from the wave theory of light, and required only one hypothesis, namely, that the product of the light action should reflect light strongly.

Although Zenker was not entirely correct and his theory did not pass unchallenged, his work eventually proved to be of great importance. Still, it was hardly appreciated at the time, and was apparently soon forgotten. Perhaps the method of its publication militated against its being widely known.

Lord Raleigh in 1887, and Prof. Otto Wiener of Aachen a few years later, both independently and without knowledge of Zenker's work, suggested that the production of standing waves might account for at least some of the results of the early colour photographs. But Wiener went a great deal further than this, and in 1895 published the results of an exhaustive examination of these methods, including the testing of pictures produced by them, to determine whether the colours were due to pigmentary matter or interference or both. He used a right-angled prism of dense glass with its three faces polished, placing the hypothenuse surface in contact with the spectrum photograph to be tested, with a liquid of high refractivity (benzene) between to exclude air, so that the refracting edge of the prism crossed the colour boundary. The colour was then viewed, with certain precautions, so that part of it was seen directly and part through the prism. With a pigmentary colour both parts remained of the same tint, but a colour due to interference was changed by the presence of the prism, so that, for example, yellow appeared bluish green. Wiener prepared some examples by Seebeck's method (precipitated silver chloride darkened by exposure to white light and then exposed to the spectrum), and found the red a rose-red, the blue greyish, and the green and yellow very poorly represented, if at all. He found these colours to be pigmentary. A modification, due originally to

Poitevin, but with slight improvements, was investigated. Silver chloride precipitated in paper was exposed to daylight in a solution of stannous chloride until darkened. It was then immersed in a solution containing potassium bichromate and cupric sulphate and exposed slightly moist. This reproduces all the colours more or less, but with a tendency towards a yellowish brown tint. These colours also are pigmentary. The colours produced by Becquerel's process, in which a silver plate has its surface converted into chloride by the electrolytic method, were found to be chiefly interference colours due to the action of standing waves, but there was pigmentary matter also present.

The colour obtained in these older processes being due entirely to pigmentary matter, except when a metal plate was used, the natural question is, What is the character of the pigmentary substance? Wiener found that silver subchloride was sensitive to all the colours of the spectrum as silver chloride that has been exposed to light is, and as these two substances are alike in other properties, there seems little doubt that silver subchloride is the colour-sensitive substance in these processes. Why it should be changed by coloured light into a substance that has some approach to the colour of the light that falls upon it is a problem still unsolved. But it seems exceedingly unlikely that any useful method of colour photography will be realised on this line of work, because the colours at their best are only approximately similar to the colour of the light that produces them; they cannot be fixed because the pigmentary matter is decomposed by solvents of silver chloride; the subchloride does not bleach to white in white light, nor is it itself black—two conditions that must be fulfilled in a material that is to reproduce colours by the direct action of light. Wiener suggested as the ideal material a black mixture of three differently coloured substances which are bleached to white by white light, and are sensitive in proportion to their absorption. In the next section the practical realisation of this principle will be described.

We have already seen that "standing waves" were predicted as a necessary result of the undulatory nature of light, and it appears that several physicists sought to obtain experimental evidence of their existence, but it was not until 1891 that Prof. Gabriel Lippmann of Paris was successful. If these stand-

ing waves could be produced in a photographic film, it was argued that deposits of silver should be obtained starting where the amplitude of vibration is greatest, with no deposit at the nodes, and that these layers would reproduce the colours that originated them when they were illuminated with white light and viewed under suitable conditions. This, therefore, would constitute a method of colour photography. It is obvious that for success the film used must be sensitive to all colours, it must have a good reflecting surface to send back the incident light, it must be transparent so that the reflected light may be as nearly as possible of its original intensity and that there may be a negligible amount of scattered light in it, and the sensitive material must be grainless, or very nearly so, for the layers of silver to be produced would be distant from each other, centre to centre, a dimension equal to only half a wave length of the light producing them. Lippmann solved the problem by coating glass with a suitable emulsion, and exposing it in a dark slide constructed to hold mercury. The glass side of the plate was turned to the light, and the sensitive film was backed up by the mercury which formed the reflecting surface. The evidence of his success lay in the facts that there was an obvious absence of pigmentary matter, the deposit being only grey silver, but that the colours of the original were reproduced as theory indicated by proper illumination and inspection, and that the colours changed by treating the film in any way that would alter the distance between the layers of silver, as, for example, by breathing on the plate and so slightly expanding the film. Perhaps the chief difficulty of this method is in ensuring that when finished the film shall be in exactly the same condition as during the exposure, or at least that the distances between the silver deposits in the finished plate shall exactly correspond with the distances between the nodes of the standing waves of the incident light. The chief drawback to the results is that they must be illuminated and viewed at suitable angles. Many workers improved the process in details, and some very excellent results have been obtained.

It still remained to actually demonstrate the existence of the laminæ of silver in the film. Dr. R. Neuhauss, Mr. E. Senior, and others afterwards, cut transverse sections of Lippmann films and obtained distinct proof of the presence of the laminæ, but by far the most complete and successful investigation of

these films has been published quite recently by Prof. S. R. Cajal of Madrid. As the resolution of the laminæ requires an objective of a numerical aperture equal to or greater than the greatest available, Cajal swelled his sections in water and so obtained excellent definition. He has photographed sections of all kinds, and analysed the effects by such methods as rubbing away the surface to distinguish between the effects of the superficial and the underlying parts. He confirms Zenker's theory exactly, except, perhaps, with regard to white. This he found was not due to the production and mixing of all colours or even complementary colours, but to simple reflection from a superficial deposit of silver; and in tinted whites or light colours he also found this superficial layer, but here it is partly transparent, and the interference colours produced by the laminæ below show through it.

Although attempts have been made, up to quite recently, to perfect the methods in which silver subchloride is the sensitive material, Lippmann's process may be regarded as the final result of all attempts to photograph colour directly. It is an exceedingly beautiful process to the experimentalist, but it has so many possibilities of error that it is very uncertain, and therefore unsuitable for general purposes as a method of photography in colours.

II

The Young-Helmholtz theory of colour vision supposes that the normal human eye is sensitive to only three fundamental colours; that is, that the points of sensitiveness of the retina, whatever they may be, are of three kinds. When white light falls upon the retina all three are excited to a definite (or equal) proportion, and when this proportion of excitation varies we become conscious of colour. Granting this, it follows that if the colour can be found that corresponds to each kind of sensitiveness, we shall have three colours that will, by their mixture, serve to imitate every possible colour, including, of course, black and white. These three fundamental colours have been determined from time to time by various investigators, and may be roughly described as red, green, and blue. We may then, for reproduction purposes, regard the light reflected from any object whatever, when illuminated in any manner

whatever, as consisting of a mixture of these three colours (or two or one of them), and by restricting the light that falls upon a sensitive plate to one of them at a time, a photograph can be made of each light-constituent reflected by the object. The photographs are themselves colourless, but they are colour records, and it only remains to bring together the three colours as indicated by the photographs to produce a picture that appears of exactly the same colours as the original—because it has exactly the same effect upon the retina. In this case no colour is produced by photography or by the photograph : the colours are pigments or dyes, and the photography merely locates them.

This is the principle of "three-colour" processes, and, speaking practically, all indirect colour photography comes under this description. It must not be thought that three-colour work is dependent upon the Young-Helmholtz theory of colour perception—it is founded rather upon the experimental facts that underlie that theory. The theory and three-colour processes are better regarded as two independent products of the same set of facts, and these facts will remain whatever fate awaits the colour-vision theory. Indeed the connection between this theory and three-colour photography may not be so intimate as it appears to some to be. The colours that represent the three sensations are fairly definite, and Colonel von Hübl maintains that the number of sets of three colours that will serve for three-colour work is indefinite. He arranges the spectrum tints in a circle with complementary colours diametrically opposite each other on a plan that would take too long to describe here, and states that the best three colours will be at the angles of an equilateral triangle drawn in the circle, and that the triangle may be in any position. At the same time he allows that practical considerations—chiefly the difficulty of getting a bright yellow by the mixing of pigments—obliges one to use the three colours which, if they do not exactly match the three primary sensation colours, approximate to them. Sir William Abney, who has done a great deal of work in this connection, and is a very successful producer of colour photographs, accepts absolutely the Young-Helmholtz theory of colour vision as his guide, and has redetermined the spectrum curves of the primary sensations. Mr. F. E. Ives, who has worked as a pioneer with very notable

success in many departments of colour photography, does practically the same, working according to Maxwell's curves. On the other hand it has been stated that the colour screens used by various workers do not always conform to their theories. It must be borne in mind, however, that the dyes and pigments used have to be selected from those that are available, and that the best of them are only more or less close approximations to what is really sought for. Thus the theories as to the selection of the colours are more diverse than the practice.

Having obtained the red, green, and blue records, there are two distinct methods by which the colours may be combined to form the picture. Each may be caused to give its appropriate colour separately, as by the use of a set of three optical lanterns, each giving one of the required colours (the photograph regulating its distribution and intensity), the three coloured images being superposed on the screen. The three colours are added together, light is added to light; the combined effect of all three is white and their absence is black. In the other method the three colour prints are superposed to form a single print or transparency. Here the combined effect of all three is black, their absence is white, and each colour subtracts whatever it can from the light—the same light that passes through all of them. In the first case the lights are added, and in the second the absorptions. The first is sometimes called the additive method and the second the subtractive method. The colours transmitted by each of the three prints in the second method must be the complementaries of those transmitted in the first case. Taking for example a red patch in the original, this will give a transparent or depositless place on the print that represents the redness of the original, which, by the triple lantern method, would give a red patch on the screen as required. But for printing or staining purposes this holds no colour, and so remains colourless. The prints that represent the greenness and the blueness of the original will be coloured at this part, the former by a colour that absorbs green and transmits red and blue, the latter by a colour that absorbs blue and transmits red and green. Thus the only colour that can get through the three superposed coloured images is the red, as required. In the additive method each image contributes one of the three colours, in the subtractive method each image subtracts or absorbs one of the three colours. These are the

general principles of three-colour photography in its two main sections.

Three-colour photography is practised commercially on a very large scale, and has been investigated and discoursed upon by a vast number of persons. A great many have dealt with the subject from a purely scientific point of view, while printers, ink-makers, and others look only at practical results. I propose therefore only to indicate the chief facts concerning its development, and to consider the most important processes that are in use at the present.

It seems that Clerk Maxwell was the first, in 1855, to suggest the principle of three-colour photography, but he used it as an illustration of the functions of the three systems of nerves according to Young's theory rather than a suggestion for practical photography. He says: "Let a plate of red glass be placed before the camera and an impression taken. The positive of this will be transparent wherever the red light has been abundant in the landscape and opaque where it has been wanting. Let it now be put in a magic lantern along with the red glass, and a red picture will be thrown on the screen. Let this operation be repeated with a green and a violet glass, and by means of three magic lanterns let the three images be superimposed on the screen . . . a complete copy of the landscape, as far as visible colour is concerned, will be thrown on the screen. The only apparent difference will be that the copy will be more subdued or less pure in tint than the original." Then he goes on to consider the eye in connection with colour vision. At the Royal Institution in 1861, Clerk Maxwell showed the experiment, using tanks of coloured liquids instead of coloured glasses, and showed the photograph of a coloured ribbon with three magic lanterns; but of course the colours were deficient because it was almost impossible at that time to photograph by red or green light.

Within the next few years others engaged themselves with the subject, but the most notable worker was Louis Ducos du Hauron, who, in 1869, published a small volume, entitled *Les Couleurs en Photographie : Solution du Problem.* He described the triple lantern "additive" method; a method of combining the three coloured images by means of mirrors; the production of prints by superposition of the coloured images—the "subtractive" method and a single-plate method—the three colours being

arranged in fine lines or otherwise on one plate. In the same year he showed some results at a meeting of the Société Française de Photographie. By a curious coincidence, at the same meeting, Charles Cros entered into great detail as to the various possible methods of trichromatic photography. But Cros appears to have contented himself with suggesting how others might succeed, while Du Hauron put his notions to the test of experiment. Moreover, it appears that Cros was wrong in his colours, recommending pale tints of red, yellow, and blue for the screens for getting the three negatives.

Work of this kind was very difficult because of the insensitiveness of the collodion plates then employed to green and especially to red light. It was not until 1873 that the sensitising effect of certain dyes upon photographic plates was discovered by Prof. H. W. Vogel. Now by this means plates can be made usefully sensitive even into the infra-red. The possibilities of special sensitising, and the introduction of gelatine dry plates about 1878, cleared away many difficulties that sorely hampered and might well have discouraged the earlier workers. From this time the development of three-colour photography was a matter of perfecting details rather than evolving new principles, and devising methods for converting laboratory experiments into commercial methods.

The additive method—the three coloured images being separately illuminated—was made into a really practical method by Mr. F. E. Ives, of Philadelphia. Mr. Ives first exhibited his apparatus in London in 1892, but four years before this he had demonstrated the capabilities of the process at the Franklin Institute in Philadelphia. In 1893 his "photo-chromoscope," afterwards called "Krōmskōp," was on the market. The apparatus that he devised for the purpose consisted of a camera for taking the three colour records, a triple lantern for projecting the picture on to the screen, and the photo-chromoscope. This last is a compact apparatus containing three coloured glasses, one for each colour record as required, and a system of mirrors, so that the three coloured images are seen superposed by looking in at the eye-piece. Shortly afterwards the apparatus was made stereoscopic. The reproductions as seen in this apparatus are remarkably realistic, and I do not think that any method has surpassed them. The one drawback is that an apparatus has to be used for seeing the pictures. Mr. Ives says

that he worked "in accordance with the Young-Helmholtz-Maxwell theory of colour vision." "Although the three fundamental colour sensations are red, green, and blue-violet, the three images of the triple photograph are not made through red, green, and blue-violet glasses, nor by the action of red, green, and blue-violet rays, but each by the joint action of all rays that have power to excite the respective fundamental colour sensations, . . . according to the measurements of Maxwell and Abney." Thus the colour screens used for taking the object each transmitted a large proportion of the spectrum. But for viewing or projecting the photographs Mr. Ives claimed to use colour screens that transmit only a narrow band of the spectrum, just that part in each case that will best excite the one sensation, and, as far as possible, that only. The disadvantage of narrow-band colour screens is that they transmit comparatively little light, and it has been stated that the screens used by Mr. Ives did not well conform to his theory. I have already referred to the uncertainty of these matters, but assuredly Mr. Ives was guided by the principles he professed, and he was remarkably successful.

The necessity for taking three separate negatives is obviously a circumstance to be avoided if possible, and the method of avoiding it was pointed out by Du Hauron in 1869. It appears that the only practicable method of making one plate suffice is to divide the surface of the plate among the three colours, using such small parts of the plate for each that when viewed in the usual way these parts merge into each other, as the fine detail of the impression of a copper or steel engraving does. The drawback to all such methods is the great loss of light. If any colour is represented by only one-third the area of the patch that stands for it, the area which might otherwise be all colour is two-thirds black. Thus two-thirds, more or less, of the light that would form the image in Ives' apparatus is lost in a one-plate process, and the picture is correspondingly duller unless the light is proportionately increased. This can be accomplished if the picture is in the form of a lantern slide, by using the same light in the lantern but reducing the diameter of the picture on the screen from 10 ft. to 6 ft. or less, or from 8 ft. to something under 5 ft.

The first successful methods of this kind were worked out independently by Dr. John Joly, of Dublin, and Mr. J. W.

McDonough, in America, from 1892 to about 1898. The later years of this period include attempts to make the process workable, so that the screens necessary might be manufactured in quantity, but in this they were not successful. The three colours were arranged regularly in lines, and were applied to the glass by means of a ruling pen, though other methods were tried afterwards. Dr. Joly followed Ives very nearly so far as his colours were concerned, except that he sought to get his colours to accord with the more recently determined sensation curves. He used two colour screens—a "taking screen" that was fixed in front of the plate while the exposure was being made, and a "viewing screen" which was put in front of the positive for seeing the picture, and might be bound up with it as a fixture if preferred. The lines were always so coarse as to be obtrusive.

Quite recently, after years of work, Mr. John H. Powrie, of Chicago, assisted by Miss Florence Warner, has so far perfected a triple-line method that he has been able to show many excellent examples. He has overcome the difficulty of making the fine coloured lines by discarding ruling methods altogether. He coats the glass with a suitable colloid made sensitive with potassium bichromate, and exposes this to light under a black-line screen that has lines twice the width of the spaces between them. The light makes the colloid insoluble in warm water, so that when "developed" like a carbon print, the coating is entirely dissolved away where it was protected by the black lines of the screen, and remains on the glass in fine lines corresponding to the screen spaces. The plate is put into a solution of a green dye; the colloid absorbs the dye, and thus the green lines are formed. The colloid is made quite insoluble to fix the dye. The plate is then coated again, and treated exactly as before; but care is taken that the black lines of the screen hide the green lines already produced, and a red dye is used. It remains to get the blue lines that shall exactly fill the spaces between the two sets of lines already obtained and without any overlapping of them. The plate is coated again, and this time exposed to light without the black line screen, but with its back to the light, so that the red and green lines on the plate prevent any light effect on the coating that overlies them and acts only upon the parts between. Warm water dissolves away the new colloid layer over the green and

red lines, leaving it exactly filling the spaces between them. The plate is put into a solution of a blue dye to stain the new lines. By this method the lines can be made more than twice as fine as by ruling (more than 600 to the inch), and the troubles of overlapping and of uncoloured gaps are entirely obviated. The method of using these plates, when they are issued to the public, will doubtless be similar to that of the Lumière "Autochrome" plates, described in the next paragraph. Their special advantages will be referred to later.

It may be here mentioned in passing that during the last twelve years it has been shown by many workers to be possible to get three colours that will serve more or less well by the use of prisms or diffraction gratings instead of dyes or pigments. But these methods do not appear to lend themselves kindly to practical work, even if no exception is taken to the principles upon which they are based.

Instead of arranging the three colours in lines, they may be disposed in small hexagon or square patches. These methods have been patented, but nothing practical has yet come of the idea. The third alternative is a random grain, and a method of doing this by means of starch granules, which was described by Messrs. Lumière, of Lyons, rather more than three years ago, has just been commercially perfected. Quantities of starch granules of approximately uniform size are stained respectively red, green, and blue, mixed as thoroughly as can be in such proportions as present a neutral grey to the eye, and dusted over a prepared plate so that they adhere in a single layer, which is then pressed or dusted with a black powder (or both) to fill up the small spaces between the granules. A protective waterproof varnish is applied, and on this is spread an orthochromatic emulsion, so that the plate is complete in itself—photographic plate and colour screen in one. It is exposed on the camera exactly as usual, except that the glass side of the plate is put towards the lens, so that the light has to pass through the layer of coloured starch granules before it affects the sensitive film. The plate is developed, and the resulting negative, instead of being fixed, has the metallic silver that constitutes the image dissolved away, and the remaining silver salt reduced to the metallic state, thus transforming the negative into a positive. When viewed as a transparency, the colours as well as the form of the original are seen. The preparation of

these plates must be regarded as a triumph of manufacturing skill. Some very excellent results have been obtained with them, but the colours are readily affected by error in their treatment. The "grain" of these plates is not so fine as one might suppose from the fact that starch granules are used, for it is apparently impossible to thoroughly mix the differently coloured granules. They occur on the plates in groups up to a dozen or more of one colour, and the groups are visible by a magnification much too low to show the individual granules.

A one-plate process such as that just described obviously must have the same dyes for taking the photograph, that is for dividing the colour of the original into the three fundamental colours, as for viewing the result, and this is a disadvantage. It is due to the wide permissible choice of colours and the non-critical comparison of the resulting colours with those of the original that the results are acceptable. These plates reproduce the colours of the spectrum very poorly. Presumably in the selection of dyes the best compromise has been made for such photography as the plates will generally be used for. It is practically impossible to multiply such colour photographs by exposing a new plate to light under a finished plate, because, as we have seen in the first plate, two-thirds of the light is lost, and the effect of making such a copy would be to lose two-thirds of the light passing through the first. Such a copy would, therefore, have about eight-ninths of its surface dark, reducing the light that would otherwise be available to about one-ninth, and it would be so dark as to be almost useless. Mr. Powrie has shown how it is possible to overcome the difficulty when using his plates, by fixing a mirror on each side of the printing-frame so that the light impinges on the plate from three directions, and then by suitably separating the two plates by a sheet of glass or celluloid every line in the upper plate—the picture—can be made to give a line of three times its width on the lower plate. So each of the three colours in the upper plate covers the whole surface of the lower plate, and no light is lost. By using light that corresponds to one colour only, it is possible by this means to get a record of that colour on an ordinary plate that is continuous—that is, that covers its whole surface instead of being as the original is, in lines that cover only one-third of its surface. Three separate continuous colour records, one for each colour, can thus be obtained from the original single

plate, and any three-colour process can be worked from these records.

The subtractive method, in which the three coloured prints are superposed, and each subtracts its quota from the light that passes through, was foreshadowed by Du Hauron and by Charles Cros. The production of transparencies in this way engaged the attention of Mr. Ives, and in 1893 he showed me a very fine example, perhaps 8 in. by 6 in. or larger. But it is to Mr. Sanger-Shepherd and his firm that we owe the establishment of this process, and the supply of the necessities for it, so that it can be worked by any one who wishes to do so. The usual three colour records are obtained; the one from the red screen is printed on a lantern transparency plate and the image is toned to a greenish blue colour, and the other two records are printed on bichromated gelatine films supported on celluloid, exposing through the celluloid. These, after development in warm water, are stained by suitable dyes, and then mounted on the greenish blue image, preferably by cementing them together with Canada balsam. One advantage of this method is that the three coloured images can be tentatively placed together, and if one colour is predominant a little of the dye can be washed out from that print, while if one is deficient it can be stained deeper. A somewhat similar method was introduced in 1895 under the name of "Pinatype" by Messrs. Meister, Lucius & Brüning. Three transparencies are prepared from the ordinary three negatives, and under each of these is exposed a sheet of bichromated gelatine. These gelatine prints are washed, and each is immersed in its appropriate dye solution. The dye is absorbed where the gelatine has not been acted on by light, but where the combined action of light and the bichromate has fully affected it, it is hardened and does not absorb the dye. These are called "print plates," for their function is to give up the dyes they have absorbed to a sheet or glass coated with a soft film of gelatine, which is pressed into optical contact with each "print plate" in turn. The "print plates" can be used repeatedly. Messrs. Sanger-Shepherd have perfected a somewhat similar method for making colour prints on paper, but their gelatine prints, which absorb the dye and then give it up to form the final picture, are developed in warm water and so form low reliefs. As "carbon" prints can be made in any colour, some of the firms that make carbon tissues have devised methods by

which three carbon prints are made of the required colours from the three colour record negatives and then superposed.

As any method by which the three coloured impressions can be superimposed on paper will give a three colour print, various printing-press methods are available, and some exceedingly fine work has been done by collotype printing, the printing surface being of hardened gelatine and inked up with printers' inks. But the process that is of all the most advantageous, especially where many impressions are wanted, consists in the preparation of three "half-tone" blocks, one corresponding to each of the three colour records, and making from these three superposed impressions in the three necessary colours. The greatest difficulty of these methods is in the getting of suitable inks, for they should not only be of the correct colour, but they (or at least two of them) should be transparent, and they should be permanent. The most suitable colouring matters are too fugitive, so theory has to be compromised, and as none are really transparent there is always a tendency for the colour last printed to predominate. In spite of these drawbacks very excellent work is done commercially by this process, but probably never without "fine etching"—that is, proof impressions from the blocks are compared critically with the original by persons who have been trained to appreciate colour and drawing; and where the tints are incorrect the alterations required are marked on the impressions, and the blocks are returned to the etcher, who works on the faulty part. In the best work this proofing and correction is repeated until the result is satisfactory. The "half-tone" block prints a series of dots equidistant from their centres and larger as the tone or shadow is deeper. In superposing the three impressions no attempt is made—indeed it would be practically impossible—to print the corresponding dots exactly over each other, and the result is that sometimes the dots are superposed and sometimes juxtaposed, according to chance. This might appear to lead to a mixture of the additive and subtractive effects, and as the colours for the one method must be the complementaries of those used in the other, it might be supposed that confusion would result. It is not so, however. Where only one ink is required, and where all three are required to their fullest extent to give black, the case is simple. Take the intermediate case of a grey produced by juxtaposition only of the three inks and in such a manner that the white paper

is completely covered. As each ink absorbs one of the three fundamental colours, we may describe the grey roughly as one-third black and two-thirds white. If now the three colours are superposed they will produce black, but only one-third of the area of the spot of white paper concerned will be covered, so that the result will be actually one-third black and two-thirds white—practically as before.

In all the processes so far considered the colour has been produced by light, or its distribution has been controlled by it. But there is a radically different method that has been in the minds of those interested in these matters for nearly thirty years, and latterly has been worked upon by many investigators with more or less, but on the whole gradually growing, success. The three necessary colours are put on the paper to begin with, and the light destroys or bleaches those that are not wanted. The method depends upon the fact that light can affect a substance only when it is absorbed, and therefore when a mixture of unstable coloured substances is exposed to coloured light, there is always a tendency for those substances that are of the same colour as the light to survive the longest, because they reflect more of the light than the others. The difficulties in working out such a method are many. The three dyes must be of the right colours, they must be about equally bleachable by white light, they must be readily bleached or the exposure would be too prolonged, and it is desirable that they shall be rendered more stable after the exposure. Dr. J. H. Smith of Zurich about a year ago put this method on a commercial footing and issued the paper ready for exposure beneath the coloured original under the name of "Uto" paper. The colours are made more sensitive by the addition of anethole, and after the exposure the print is soaked in benzene or acetone to remove the sensitiser. This paper gives surprisingly vivid reproductions of the colour of the original, but obviously the prints are not very stable to light.

Thus the present practical methods of colour photography are "three-colour" methods, and they do not aim at reproducing colour, but only at imitating it. If the imitation is perfect by daylight it will probably be faulty by artificial light, and a spectroscopic analysis of the two colours will at once show their differences. The perfection of the imitation depends upon the skill of the worker as well as upon the characteristics of the

method. The colour of the photograph is not impersonal evidence of the colour of the original; but the same qualification applies to the vast majority of observation records. This much, however, may be said in favour of three-colour processes—if a series of photographs is made by the same process and with the same materials, the discrepancies of colour would probably be more easy to trace than the random errors possible when the colouring is done entirely by hand. Moreover, any one who is accustomed to photographic manipulations and others with practice can work some of the processes, while it must be the comparatively few who can ever hope to become skilled in applying colours with the brush.

THE EFFECTS OF ENVIRONMENT AND NUTRITION UPON FERTILITY

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EXPERIENCED breeders of animals have seldom failed to recognise that the generative system is particularly susceptible to changed conditions of existence. Buffon long ago commented on the fact that the domestic animals are as a rule far more prolific than their wild representatives, and Darwin, who made the same observation, was disposed to ascribe the increased fertility of the former to a long habituation to a copious food supply without the labour of seeking for it. On the other hand, it is equally well known that wild animals when brought into captivity often become sterile, while there are numerous instances also of domestic animals failing to breed as a result of removal to a new environment. Such sterility, when it occurs, is not necessarily (or even generally) associated with disease or want of vigour, for, as Darwin observed, wild animals in confinement are not infrequently notoriously healthy and long-lived, and nevertheless fail consistently to produce young. There is no evidence that the reproductive organs are themselves diseased, yet they seem incapable of performing their natural functions. Moreover, in those cases in which wild animals in captivity have proved fertile, the capacity to breed is not necessarily inherited by their offspring, although, as a general rule, the fertility tends to increase in subsequent generations. Thus, it was a considerable time before the canary bird was fully fertile, but it eventually became so, whereas certain of the nearly related finches, while producing hybrids with the canary, only occasionally reproduce their own kind when kept in captivity. Other wild birds again, such as most members of the great families of ducks, pigeons, and fowls breed as readily in confinement as in their natural state. Similar differences occur in the various groups of mammals. For instance, bears breed less freely in the Zoological Gardens

than most other carnivores, although it is difficult to assign a reason for this fact. Furthermore, it was pointed out by Darwin that close confinement cannot be regarded as a sufficient explanation of infertility in such cases, since the semi-domesticated ferret, for example, breeds readily when kept in closely cramped hutches.

There can be no doubt that those environmented conditions which are conducive to the occurrence of "heat" or œstrus are generally also favourable to fertility, for although it is true that many animals which have never succeeded in producing offspring in captivity have been observed to experience œstrus and indulge in sexual intercourse, there can be no question that a prolonged or frequently recurring sexual season is usually associated with an increased capacity to bear young. The origin of the breeding season has been discussed in two papers by Mr. Walter Heape,[1] who lays considerable stress on the part played by nutritive influences, as shown especially by the effects of certain stimulating foods upon the generative functions of sheep. But it is none the less evident that nutrition is not the sole factor in determining the time for breeding, for, as Mr. Heape points out, its occurrence is to a great extent seasonal, and is governed both by external forces (*e.g.* climatic influences) consequent upon periodic change, and by internal forces dependent on the inherited capacity of the individual.

Some interesting observations made by Mr. Bles on various kinds of Amphibia seem to have a bearing upon this question.[2] This observer shows that axolotls, as well as certain species of frogs and newts, can only be induced to breed in captivity in the presence of particular environmented conditions. Mr. Bles found that by feeding them copiously in summer and allowing them to hibernate in winter, and afterwards suddenly transferring them to an aquarium stocked with growing plants and provided with running water or with suitable pond water, these animals could be induced to spawn within a few days. He draws the conclusion that the difficulty so often met with in inducing Amphibia to breed is not due to any toxic influence on the gonads resulting from close confinement, but should

[1] Heape, *Quart. Journ. Micr. Science*, vol. xliv. 1900; *Proc. Royal Soc.* B, vol. lxxvi. 1905.

[2] Bles, *Trans. Royal Soc. Edinburgh*, vol. xli. 1906.

rather be ascribed to the absence of the necessary external stimuli without which the generative organs of animals are incapable of discharging their functions. This view may also help to explain why some animals (*e.g.* insects) make their appearance in great numbers in one year and are comparatively scarce in another.

In those animals which, as a general rule, breed readily in a state of domestication or confinement, it is probable that nutrition plays the most important part in regulating the capacity to produce offspring. That an insufficient or markedly abnormal diet must affect this power is almost self-obvious. It is also clear that the effects of excessive nutriment are likewise prejudicial to the proper discharge of the reproductive functions. No better example could be given of the way in which over-feeding results in a condition of sterility than that afforded by the barren Shire mares which have been a noticeable feature at recent agricultural shows. Breeders of cows and sheep have learnt from long experience and at great cost the disastrous consequences of an over-nourishing diet upon the fertility of their stock. It would seem, however, that owing to the desire to excel in the showyard breeders of draught horses are falling continually into the old error. In the classes provided for barren Shire mares there are invariably to be found some of the finest specimens of the breed, so that those animals which appear best calculated to produce a superior class of foals are useless for stock-breeding purposes. Little or nothing is certainly known regarding the way in which excessive feeding checks the tendency to conceive, but this result may probably be ascribed to fatty degeneration in the organs of reproduction.

It is a somewhat remarkable fact that, although numerous experiments have been undertaken to determine the effects of different methods of treatment upon wool, meat, or milk production, no systematic attempt had been made until quite recently to deal with the factors which influence fertility in livestock. A few years ago, however, the Royal Agricultural Society instituted an inquiry into the causes of barrenness and abortion among different breeds of sheep in the south of England. The investigation was conducted by Mr. Heape, whose report was subsequently published in the *Journal* of the Society.[1]

[1] Heape, "Abortion, Barrenness, and Fertility in Sheep," *Jour. Royal Agric. Soc.* vol. x. 1899.

In this report it is shown that in the season 1899 the proportion of ewes which failed to breed or which aborted their lambs was 6·76 per cent., and that the number which produced twins was about 30 per cent. The most fertile of all the breeds was the Wensleydale, in which six flocks consisting of a total of 319 ewes produced a percentage of 177·43 lambs. The effects of locality are also discussed, and there is an accumulation of evidence which seems to indicate that the character of the district is not without influence on the fertility of the breed. Thus the records show that whereas the sheep belonging to the Lincoln variety in Yorkshire produced an average of 47·57 per cent. of twins, those in the home county had only 25·11 per cent., while those in neighbouring counties produced 33·78 per cent. With other breeds the variation in the fertility in different districts, while quite noticeable, was not so pronounced as in the Lincoln sheep.

The report shows, further, that the fertility of a flock depends greatly upon its management, that the quality and quantity of the food supplied affect the condition of the sheep and so influence their capacity to breed, that some seasons are far more favourable to fertility than others, and that sheep-stained pasture (*i.e.* pasture on which sheep have recently been kept for some little time previously) is often prejudicial to breeding stock.

As a result of a recent investigation upon the reproductive processes in sheep, it has been shown that (in the breeds investigated) the normal percentage of Graafian follicles discharging their ova at any single "heat" period does not appreciably exceed the usual percentage of births at the lambing season. It would seem, therefore, that a comparative scarcity of twins at lambing time is almost certainly directly correlated with an abnormally small number of ripe follicles in the ovary at tupping time (*i.e.* during the breeding season). An unusually low percentage of twins is closely associated with barrenness. This fact is generally recognised by flockmasters, and Mr. Heape's statistics prove it very clearly. And since habitually (*i.e.* constitutionally) barren ewes are a rarity, there can be little doubt that barrenness is due normally to the same cause operating on a more exaggerated scale—*i.e.* to the absence or great scarcity of Graafian follicles available for ovulation during the tupping season.[1]

[1] Marshall, *Phil. Trans.* B. vol. cxcvi. 1903.

Scarcity of ripe follicles at tupping time must clearly result either from retardation in follicular development or from an unusual amount of follicular degeneration, either recently or at some previous period in the animal's lifetime. Degeneration of follicles is by no means uncommon in the ovaries of sheep, and appears to be most frequent in those which have attained to from one-eighth to one-half the dimensions of the mature follicle. It may set in, however, at practically any stage of development. The follicle, instead of continuing to grow, undergoes a series of retrogressive changes, which result in the shrivelling up of the ovum and the ultimate absorption of the entire contents of the follicle. There can scarcely be any doubt that the degeneration is usually the result of an insufficiency of stimulating power at the disposal of the ewe.

That scarcity of mature follicles in sheep's ovaries at tupping time may be due to retardation of growth is a conclusion that is based on inference rather than on direct observation, for little is known concerning the rate of development of the Graafian follicle. There is every reason for believing, however, that the process of ripening can be very largely influenced both by insufficiency of food supply on the one hand, and by artificial stimulation on the other.

This fact has been perfectly well realised for years past by certain individual flockmasters, who have consistently practised the methods of flushing or artificially stimulating their sheep by means of an extra supply of special food at the approach of the breeding season. It is somewhat surprising, however, that no precise records of the effects of flushing upon fertility had been collected until quite recently, when the Highland and Agricultural Society of Scotland undertook an inquiry upon this subject. The absence of published information may perhaps explain the prevalent ignorance of the results of flushing among many sheep-breeders who have never adopted it.

In a preliminary report upon the Highland Society's investigation[1] containing the lambing statistics for various Scottish breeds in the year 1905, it is shown that the percentage of lambs born was, as a rule, largest among flocks which had been subjected to a process of artificial stimulation. The method adopted was to feed the ewes upon turnips, oats, dried grains, maize, or other artificial food at the tupping time and for about

[1] Marshall, *Proc. Royal Soc.* B, vol. lxxvii. 1905.

three weeks previously, while maintaining them upon grass only during the greater part of the year. Some flocks, however, received a limited quantity of additional food (turnips, etc.) during pregnancy, and especially during the latter part. The statistics show that in the flocks which were treated in this way the percentage of lambs per ewes[1] was considerably in excess of the average percentage for flocks which received no special treatment, while the percentage of barren ewes was generally appreciably less. In some cases the number of lambs per ewes[1] in the flushed flocks was nearly 200 per cent. Among flocks of the same breeds (Border Leicester or half-bred Border Leicester) which received no sort of special treatment, the average proportion of lambs per ewes[1] was between 150 and 160 per cent., while flocks which were placed upon superior pasture at the approach of the breeding season (without being otherwise specially fed) generally produced a slightly larger percentage of lambs (about 160 per cent.).

It may be of some interest to describe more precisely the method of treatment adopted for certain of the flushed flocks in which a relatively large number of twin lambs were produced.

In one of these the ewes (which were half-bred Border Leicester by Cheviot) were fed upon grass only during the summer previous to tupping. For three weeks (during tupping, beginning of October) they were allowed a full supply of turnips upon grass. Between tupping and lambing (five months) they were given a mixture of dried grains and turnips, and lamb food for three weeks before lambing. The rams (which belonged to the pure Border Leicester breed) were fed upon bruised oats during tupping. The percentage of lambs per ewes[1] was 191·5. One ewe had four lambs and 12·5 per cent. had triplets. None of the ewes were barren and none aborted.

In a second experiment the ewes (which were also half-bred), after being kept upon grass during the summer, were supplied with Bombay cake, bruised barley, and a little linseed, as well as turnips and cabbages during the tupping season. Some turnips were given during pregnancy. The rams (Border Leicester and Oxford Down) were treated in the same way. The percentage of lambs per ewes[1] was 193·75. None of the ewes aborted and none were barren. Triplets were produced in 13·5 per cent. of the ewes.

[1] That is to say, the number of lambs per 100 ewes.

In a third case the ewes (half-bred) were placed upon superior pasture at tupping, and from that time onwards until lambing they were supplied with hay and turnips. Previous to tupping they were fed upon grass alone. The rams (Border Leicester, Oxford Down, and Cheviot) were treated similarly to the ewes before and during tupping. The percentage of lambs per ewes[1] was as high as 196. Out of 184 ewes 23 had triplets. Two ewes aborted and one was barren.

In another experiment 60 ewes were placed upon rape and seeds at tupping time. Of these, 54 produced either twins or triplets at the following lambing, the proportion far exceeding that in the other sheep which were upon the same farm but not similarly treated.

The Highland Society's investigation is still in progress, and a full report will not be issued until next year. In the meantime it may be stated that further experiments fully confirm the conclusion that stimulation of the generative organs of sheep by a system of special feeding at the beginning of the breeding season results in an increased crop of lambs at the following lambing season. The twins appear almost invariably to have been born early during lambing, thus showing that the reproductive activity of the ewes is usually greatest at the beginning of the breeding season.

There is abundant evidence also that flushing tends to hasten the time for breeding. It has been shown recently that the condition of "heat" in animals is probably brought about through the action of an internal secretion or chemical excitant elaborated by the ovaries.[2] It would seem, therefore, that the artificial feeding exercises a stimulating influence over the secretory activity of the ovaries, while at the same time promoting the more rapid growth and maturation of the follicles, and causing a larger number to discharge their ova at the earlier "heat" periods of the breeding season.

It has so far proved more difficult to obtain precise information regarding the effects of flushing in one season upon the fertility of the sheep in after-years. The more usual experience of those flockmasters who have practised flushing appears to be that this process is not in any way detrimental unless it is over-

[1] That is to say, the number of lambs per 100 ewes.

[2] Marshall and Jolly, *Phil. Trans.* B, vol. cxcviii. 1905. Cf. also Knauer, *Arch. f. Gynäk.* vol. lx. 1899; and Halban, *S.B. Akad. Wiss.* Wien, vol. cx. 1901.

done. If the artificial feeding is excessive, and the sheep are made to depend for the rest of the year (*i.e.* during gestation and afterwards) upon nothing better than sustenance diet, one might reasonably expect the ewes to deteriorate, and their subsequent fertility to be impaired. It is apparently for such a reason as this that some breeders, especially in England, regard the practice of flushing as altogether to be deprecated. In this connection it should be mentioned that there is some evidence for the belief that if sheep are flushed in one year, the process must be repeated in the next, and that if this practice is not adopted, the ewes tend to be less fertile than if they had never been flushed at all.

On the other hand, certain of the reports show that sheep which produce twins one year, in a large proportion of cases bear twins also in the year following. This appears to occur irrespectively of flushing. It would seem, therefore, that an increased degree of fertility is a characteristic of certain particular ewes.

Before concluding this brief summary of recent investigation upon certain of the factors which control fertility, I may take this opportunity of urging the importance of further inquiry in the interests of the livestock industry. In these days, when much is heard of agricultural depression, it is not always realised that the breeding of domestic animals is one of the staple industries of the country. The prominent position which British horses, British sheep, and British cattle still hold in the livestock markets of the world is, perhaps, the most hopeful indication for the future of British agriculture. It is a sufficiently remarkable fact that, excepting for the Holstein cattle, the Percheron horses, and the Merino sheep, all the prominent breeds of livestock throughout the world have their foundation in animals of British ancestry.

Even the Percheron horses are not the equals of the English Shires for stamina and power. Nearly every good horse on the Continent and in America is derived from British stock. The famous American trotters are descended from English hackneys. At the international conference of sheep-breeders which recently met at Ipswich, great stress was laid upon the high favour in which British sheep are always held by the great cosmopolitan buyers. The livestock export from Great Britain goes on increasing year by year, and last year was phenomenal,

reaching a grand total value of a million and three-quarter pounds.

And yet, despite its comparative prosperity, it is very evident that the breeding industry suffers annually from no inappreciable loss. The prevalent sterility among the better class of Shire mares has already been alluded to, while incapacity to breed is perhaps still commoner among Thoroughbreds, for the Royal Commission on Horse-Breeding showed that no less than 40 per cent. of the country's thoroughbred mares fail to have foals each year. Among cattle the annual loss from sterility has been estimated at 15 per cent., while it is shown in the report of the Royal Agricultural Society on fertility in English sheep for the year 1899 that the proportion of ewes which failed to breed was over 6½ per cent. In view of the facts of which these are only examples, it is sufficiently obvious, as Mr. Heape has pointed out, that any means by which sterility can be reduced and the capacity to bear young increased must be possessed of great commercial value. And this result can only be attaiend by continuous scientific investigation and the introduction of scientific method into all the problems of practical breeding.

Mr. Heape, in his work on *The Breeding Industry: Its Value to the Country and its Needs*, has shown from statistical evidence that the amount of money invested in livestock in this country must be computed at scarcely less than £450,000,000. Adding to this the capital spent on buildings, land, vehicles, and various accessories, the total becomes still more gigantic. And yet what Mr. Bateson said at the Cambridge meeting of the British Association three years ago is still almost true: "Breeding is the greatest industry to which science has never yet been applied." It cannot be doubted that the introduction of rigorous scientific method into the breeder's industry will be attended by immense advantage, just as it has proved invaluable to other industries.

WORK UNDER PRESSURE AND IN GREAT HEAT[1]

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The development in all directions of engineering and manufacturing work is constantly bringing man into new and unaccustomed relations with his environment. In consequence of this, his working efficiency may be reduced very greatly, and his health, or life itself, may be endangered. The question of how to carry on work safely and efficiently under an unwonted environment is thus one which occurs in many forms to those who are engaged in our branch of science. In the present address I wish to glance shortly at several problems of industrial hygiene, presented by work beneath water or beneath the ground, and at some recent attempts at their investigation and practical solution.

I shall refer first to work in deep water under the sea. In consequence of the difficulties and dangers connected with the work of divers on the sea-bottom, the British Admiralty two years ago appointed a Committee to investigate the subject, with myself as the physiological member. The experiments carried out by this Committee, and at the Lister Institute, London, in close connection with it, are now completed; and I propose to give some account of the results, which, like many other British investigations on questions of hygiene, are hidden under the somewhat forbidding form of an official blue-book.[2]

Let me remind you that the ordinary diving dress consists of a copper helmet screwed to a corselet, the latter being in its turn connected water-tight to a stout waterproof dress covering every part of the body except the hands, which project through elastic cuffs. Air is supplied through a non-return valve on the helmet from a flexible pipe connected with an air-pump on a boat or

[1] An address delivered at the General Meeting of the International Congress of Hygiene, Berlin, September 26th, 1907.

[2] *Report to the Admiralty on the Conditions of Deep Diving*, 1907.

ship: the air escapes through an adjustable spring valve at the side of the helmet. The arrangement is thus such that the pressure of the helmet air breathed by the diver is always equal to, or slightly greater than, the pressure of the water at the valve outlet. At a depth of 10 metres (33 ft.) the diver is therefore breathing air at an excess pressure of 1 atmosphere, or at an absolute pressure of 2 atmospheres; and every additional 10 metres will add another atmosphere to the pressure.

Provided that about 50 to 100 litres of air per minute are supplied to the diver, and that the outlet valve of his helmet is properly adjusted, he can work quite comfortably at depths up to about 20 metres. Beyond this depth, however, he usually feels increasing discomfort and difficulty in working; so that at 40 or 50 metres work becomes extremely difficult. This difficulty has hitherto been ascribed to the pressure: its real cause was one of the first points which we investigated. We found that the discomfort of the diver was associated with laboured or dyspnœic breathing; and this fact, in connection with the results of recent experiments on the normal regulation of breathing, suggested the true explanation.

In a paper published two years ago by Mr. Priestley and myself,[1] it was shown that under normal conditions the breathing is always regulated in such a way as to keep the partial pressure of the carbon dioxide in the air of the lung alveoli almost exactly constant. This fact was established by analysing samples of air from the lung alveoli in man. To obtain a sample all that is necessary is to make a sudden deep expiration (during perfectly natural breathing) through a long piece of tubing of wide bore, and at once withdraw into a gas-analysis apparatus a portion of the air left in this tube at the end of expiration. The deep expiration washes out all the more or less pure air contained in the air-passages and tube before the deep expiration, and leaves the tube full of pure alveolar air. This pure alveolar air contains for each individual an almost astoundingly constant percentage of CO_2 if the barometric pressure is constant. If the barometric pressure varies, the percentage of CO_2 varies inversely as the barometric pressure, so that the partial pressure of CO_2 remains constant. For instance, we found in ourselves that the mean percentage

[1] *Journal of Physiology*, vol. xxxii. p. 225, 1905.

of CO_2 in our alveolar air was 6·01 at normal pressure, and 3·53 in compressed air at 1·7 atmospheres absolute pressure. The corresponding partial pressure of CO_2 in the alveolar air was 5·68 per cent. of an atmosphere, or 43 mm. of mercury, during respiration at normal pressure, and exactly the same in the compressed air. Thus the respiratory centre normally responds only to variations in the partial pressure of CO_2 in the blood which irrigates it. To variations in the partial pressure of oxygen it is within very wide limits absolutely indifferent. Only when the partial pressure of oxygen in the air inspired falls below about two-thirds of the normal does the respiratory centre begin to respond to want of oxygen. Quite recent experiments by Hill and Greenwood at high pressures in air, and by Boycott and myself at low pressures in a mixture of oxygen and air, have shown that at an absolute pressure of 6 atmospheres the alveolar CO_2 percentage was 0·9, and at 0·4 atmospheres it was 15; while at normal pressure it was about 5·5 with the same individuals. The partial pressure of CO_2 (*i.e.* the percentage in the moist alveolar air multiplied by the barometric pressure) remained, however, quite constant.

If CO_2 is present in the air inspired, the effect is to make the breathing deeper, and finally also more frequent; unless, however, the pressure of CO_2 in the inspired air begins to approach the normal alveolar CO_2 pressure, there is very little change in the alveolar CO_2 pressure, compensation being produced very easily.

How, now, does this apply to the case of the diver? On account of the work involved in pumping, a diver has to content himself with a minimum supply of air. Let us suppose that when he is just under water he has sufficient air to prevent the CO_2 percentage in the helmet air from rising beyond 3 while he is at work. This will keep him fairly comfortable. If he now goes down 10 metres, the air supply being the same, the percentage of CO_2 in the helmet air will also remain the same. As, however, the pressure is now 2 atmospheres, his normal alveolar CO_2 percentage will be 2·8 instead of 5·6. It will, however, be quite impossible for him to maintain this percentage of CO_2 in his alveoli, since during work the percentage in the inspired air itself is 3. He will thus suffer from severe dyspnœa. A very little consideration will show that the diver must have twice as much air at 10 metres if he is to be as comfortable as he

was at surface; and at whatever depth he may be his minimum air supply must be increased in proportion to the increase in the absolute air pressure. In other words, the minimum *volume* of air supplied to him, measured at the pressure he is under, must always remain the same.

To cut a long story short, we found that as soon as this condition was realised, as proved by numerous analyses of air from the helmet, the discomfort and loss of working power in deep water disappeared. Lieutenant Damant and Gunner Catto, the two officers who carried out most of the experiments in deep water, and who reached the greatest depth hitherto definitely recorded, were as free from respiratory distress at 64 metres (210 ft.) as at 1 metre. They could also reach this depth easily within two minutes, which showed the extraordinary fallacy in the time-honoured tradition that a diver should always descend slowly, so as to accustom himself by degrees to the change of pressure.

By guarding against excessive increase of CO_2 pressure in the air breathed by a diver, we not only increase greatly his working efficiency and comfort, but also obviate a serious danger. It not infrequently happens that a diver, particularly if he is somewhat unskilled, becomes unconscious or stupefied by the effects of CO_2. The consequences of this may easily be fatal, as it is very dangerous to draw a man up rapidly from a great depth if he has been down for any time; and also dangerous to leave an unconscious man on the bottom.

In connection with the effects of CO_2 in a diver's helmet, I should like to say something as to the presence of CO_2 and other impurities in the air of mines. It is well known that an excess of CO_2 and corresponding deficiency of oxygen is commonly met with in the air of mines, and that certain kinds of mining work are very unhealthy—particularly the mining of some of the more valuable metals, such as gold, tin, silver, copper, and lead. On account of the high death-rate and large amount of illness among the tin-miners of Cornwall in England, I was asked in 1902 by the British Government to investigate the ventilation of the mines there. It was known from the results of an earlier inquiry,[1] confirmed by my own analyses, that it is easy enough to find many working places in these mines where the proportion of CO_2 rises to as much as 50 volumes per

[1] *Report of the Royal Commission on Metalliferous Mines*, 1864.

10,000 of air, or higher; and much stress had been laid on the gaseous impurities of the air as explaining the ill-health of the miners. The physiological data already mentioned prove, however, that although an actual rise of ½ or 1 per cent. in the alveolar CO_2 pressure would have most violent physiological effects, no such rise actually occurs when air containing ½ or 1 per cent. of CO_2 is breathed. The only effect is that the breathing becomes very slightly deeper, the difference being subjectively quite inappreciable. Less than 1 per cent. of CO_2, or 100 volumes per 10,000, is therefore of no importance in air at ordinary pressure, provided no other harmful inpurity is present. Except in exceptional cases—for instance, just after blasting—there was no evidence of any other harmful gaseous impurity in the mine air; and by far the greater part of the CO_2 was found to arise from the slow oxidation of mineral matter such as iron pyrites (FeS_2), with consequent liberation of CO_2 from carbonates by the sulphuric acid formed.

What, then, was the cause of the ill-health of the Cornish miners? Further examination[1] into the facts disclosed two causes. The first was that certain of the mines were badly infected with ankylostomiasis, which, as has so often been the case during the last hundred years, had not been recognised, and which was soon brought under control by suitable measures. The second, and by far the more serious, cause was the fact that dust raised locally by drilling and blasting granite, quartz, and other hard stone was being inhaled by the miners and causing injury to their lungs which produced a marked predisposition to tubercular phthisis. The increased death-rate among the miners was due entirely to lung disease, chiefly tubercular phthisis. During middle life, for instance (age thirty-five to forty-five), the death-rate from lung disease was 33 per 1,000 among Cornish miners, as compared with 3·4 per 1,000 among coal and iron miners; and from all other causes 5·8 per 1,000 among Cornish miners, and 6·5 per 1,000 among coal and iron miners. An investigation of the working history of each man who had died in one of the mining districts during a period of three years revealed the fact that the extra deaths—at any rate during middle life—were confined almost entirely to men who had been engaged either locally or

[1] *Report on an Outbreak of Ankylostomiasis in Cornish Mines;* and *Report on the Health of Cornish Miners*, Parliamentary Papers, 1902 and 1904.

abroad in the dustiest work—namely, the management of machine drills worked by compressed air. By the use of water jets or other means, which I need not specify in detail, the inhalation of stone-dust from these drills and from blasting can be entirely prevented; and legislation rendering effective means compulsory was carried through at once with the full support of the management of the mines.

The accompanying tables show some of the Cornish statistics:

TABLE I

Annual Deaths per 1,000 *living at each age*

	Ages					
	15—20	20—25	25—35	35—45	45—55	55—65
All miners (Cornwall):						
Lung diseases	0·7	2·7	17·3	33·2	32·2	42·6
All other causes... ...	1·0	3·1	3·6	5·8	12·4	27·9
Coal miners (England):						
Lung diseases	0·9	2·0	2·1	3·5	7·8	18·7
All other causes	2·9	3·6	4·2	6·5	11·6	25.1
Ironstone miners (England):						
Lung diseases	1·6	1·5	2·1	3·2	6·5	13·0
All other causes	1·8	3·1	3·8	5·0	10·6	20·3
All occupied males (England):						
Lung diseases	0·95	2·7	3·7	5·9	8·6	13·0
All other causes	1·6	2·4	3·6	6·5	12·9	20·7

TABLE II

Deaths of all men who had ever worked Machine Drills, and who died in Redruth District, Cornwall, 1900-2

	Ages					Total.	Mean Age at Death.	Mean Duration of Machine Drill Work.
	15—25	25—35	35—45	45—55	55—65			
Lung diseases	3	51	54	22	3	133[1]	37·2	7·0 years
All other causes	2	4	1	2	0	9	—	—

It is a remarkable fact that while the inhalation of dust from hard stone is extremely dangerous when habitually inhaled, the

[1] Of these, 122 were returned as phthisis.

dust from coal, or soft stone, and many other materials, has no such effect, at least in moderate quantities. An average English coal-miner inhales very large amounts of coal-dust; but coal-mining is one of the healthiest of all occupations, in spite of absence of light, exposure to extremes of temperature, and the inhalation of air which is, chemically speaking, far from pure. It just happens that the ordinary impurities—moderate excess of CO_2, CH_4, dust from coal and shale, and deficiency of oxygen—matter comparatively little from the health point of view, whereas dust from hard stone matters a great deal.

I must now return to deep diving. The most formidable danger presented by this occupation occurs only on the ascent of a diver to surface. It has been known for long that divers and other workers in compressed air are subject on their return to surface to a variety of serious symptoms—in mild cases only pains in the limbs or elsewhere (usually known among English workers in compressed air as "bends"); in more serious cases paralysis, either temporary or permanent, particularly of the legs and bladder; or, finally, symptoms of asphyxia, which may prove rapidly fatal. The classical experiments of Paul Bert[1] proved, thirty years ago, that all these symptoms are due to liberation of gas-bubbles in the blood or elsewhere within the body; and considerable further light has been thrown on this subject by more recent work, including particularly that of Catsaras, von Schrötter,[2] and Hill. It was on the prevention, rather than on the pathology, of these symptoms, known collectively as caisson disease, that the Admiralty Committee concentrated its attention. The blood and body tissues become saturated with gaseous nitrogen in accordance with Dalton's Law during exposure to compressed air. On rapid decompression this gas liberates itself in the blood as bubbles, which cause local or general blocking of the circulation, with corresponding symptoms. Paul Bert showed on animals that in order to avoid this condition decompression must be slow; but the question, How slow? had not been satisfactorily answered in the case, at least, of man. The nearest attempt at

[1] *La Pression Barométrique*, 1878.

[2] Heller, Mager, and von Schrötter, *Die Luftdruckerkrankungen*, Vienna, 1900; von Schrötter, *Der Sauerstoff in der Prophylaxie und Therapie der Luftdruckerkrankungen*, 2nd ed., Berlin, 1906.

an answer was that of von Schrötter and his colleagues, based in part on theoretical considerations suggested by Zuntz, and evidently to a large extent correct. In many points, however, it seemed to us that von Schrötter's conclusions were open to criticism. Practical experience, for instance, seemed to show that it takes far longer than he believed for the human body to become saturated with nitrogen in compressed air, and a correspondingly longer time to become desaturated. Similar criticisms applied to the conclusions of Hill and his colleagues on this subject.[1]

It was evidently necessary to make further experiments, and these were rendered possible in England by the gift to the Lister Institute by Dr. Ludwig Mond, F.R.S., of a large experimental steel chamber capable of comfortably taking a number of animals or several men. In this the experiments were carried out by Dr. A. E. Boycott, of the Lister Institute, Fellow of Brasenose College, Oxford, and Lieutenant Damant, the naval officer who, as already mentioned, was also engaged in experiments on himself and others in deep water at sea. For the experiments on animals goats were selected, as we were convinced from the outset that small animals, in consequence of their rapid circulation, were useless for our purpose. Indeed, very small animals showed no symptoms whatsoever after exposures from which rapid decompression killed goats with certainty, and *a fortiori* would have killed men. With long exposures to high pressures the risks to be guarded against on decompression are undoubtedly still greater in men than in goats.

Several hundred decompressions of goats, men, and other animals have been carried out during 1906 and 1907 by Boycott and Damant. In the time available I cannot, however, give more than an outline of some of the results.

In the first place it was found that, with equal conditions as regards decompression, the symptoms observed in goats increased in danger and severity as the duration of exposure increased up to at least two hours or perhaps somewhat more. This is illustrated in Table III., showing some of the results:

[1] *Journal of Hygiene*, vol. iii. p. 436, 1903 ; *Proceedings of the Royal Society*, vol. lxx. p. 455, 1903 ; vol. lxxvii. p. 442, 1906 ; vol. lxxix. p. 21, 1907.

TABLE III

Absolute Pressure 4 *Atmospheres*

Exposure in Minutes.	Decompression in Minutes.	Number of Goats.	Cases of "Bends."	Severe Symptoms.
15	1	15	1	0
30	1	15	3	0
60	1	14	4	0
120	1	10	2	4
60	10 (uniform)	11	3	1
120	10 „	11	4	1
240	10 „	11	4	1
480	10 „	11	3	2

Allowing, therefore, for the difference between goats and men in the respiratory exchange per unit of body weight, and consequent rate of circulation, it would take about four hours for a man's body to become for practical purposes saturated with nitrogen in compressed air. This conclusion harmonises with the experience of workers in compressed air. Parts in which the circulation is rapid are certainly saturated in a small fraction of this time, as indeed has been shown experimentally by Hill by determining the amount of free nitrogen in urine secreted during rapid diuresis.[1] We think, however, that von Schrötter, who calculated that saturation is nearly reached in man within about twenty minutes, has at any rate made a great *practical* underestimate, both of the time needed for saturation and that needed for desaturation. In making his calculation, he has treated the body as if the circulation through all parts were the same, whereas it differs very greatly in different parts. From the point of view of caisson disease, the quickly saturating parts cause little danger (except with almost instantaneous decompression from very high pressure), as is shown both by the practical experience of men working in compressed air, and by the fact that it is extremely difficult to produce any symptoms in small animals with a rapid circulation rate. It is thus the slowly saturating parts of the body which are of most practical importance. Another source of error is that, as was shown by Dr. Vernon,[2] of Oxford, in connection with our experiments, fat

[1] *Proceedings of the Royal Society*, vol. lxxix. p. 366, 1907.
[2] *Ibid.*

takes up about six times as much free nitrogen as does blood. The body fat thus forms a very large depôt of nitrogen, which greatly increases the time needed for saturation. On sudden decompression of an animal saturated with compressed air the fat becomes blown out with small gas-bubbles.

We are in complete agreement with von Schrötter and Zuntz as to the general form which the saturation curve of any part of the body must of necessity take, assuming the rate of circulation to remain constant (see fig. 1). For instance, a part which takes one hour to become half-saturated will take two hours to become three-fourths saturated, and three hours to become seven-eighths saturated. During desaturation on decompres-

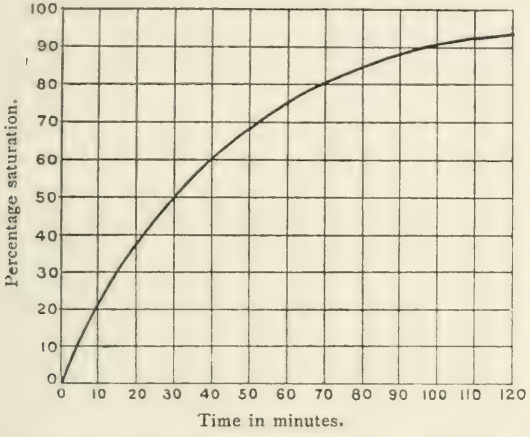


FIG. 1.—Curve showing approximate rate of saturation of parts of the body with an average rate of circulation and percentage of fat.

sion, if no bubbles have formed to disturb the process, the desaturation curve will be exactly the same.

I now come to another point, as to which we have been led to new and important conclusions. Practical experience of work in compressed air shows that even with very rapid decompression no symptoms of caisson disease ever occur with an absolute pressure of less than 2 atmospheres, and that symptoms are very rare and slight until the pressure rises beyond 2·3 atmospheres, or 19 lb. per square inch. This we found to be true also for goats. Now if it is possible to decompress rapidly and with safety from 2 atmospheres, or a little more, to 1 atmosphere, it seemed likely that it would be possible to decompress with

equal safety from 4 atmospheres to 2, or from 6 to 3, since the volume of gas tending to be liberated would be the same in each case. Experiment showed that this was the case, and that the danger of rapid decompression depends, not on the absolute difference between the initial and final pressure, but on the proportion between the two pressures. If this proportion is only 2 or 2·3 to 1, the decompression is safe; if, on the other hand, the proportion is 3 or 4 to 1, the decompression is dangerous. This is illustrated by the experiments in Table IV.

TABLE IV

Absolute Pressure in Atmospheres.	Exposure in Minutes.	Fall of Pressure in Atmospheres.	Relative Reduction of Pressure.	Duration of Decompression in Minutes.	Number of Goats.	Cases of "Bends."	Severe Symptoms.	Death.
6	180	3·4	2·3 to 1	1½	10	0	0	0
4·4	180	3·4	4·4 „ 1	4	10	3	3	2
4	120	3·4	6·7 „ 1	6	3	1	1	1
3·6	120	3·0	6 „ 1	6	4	0	3	0
4	120	3·0	4 „ 1	1	10	2	4	0

It will be seen that a rapid drop from 6 to 2·6 atmospheres produced no symptoms at all, the fall of pressure being 3·4 atmospheres, and the relation of the higher to the lower pressure being 2·3 to 1. With the same animals decompression from 4·4 atmospheres to 1 produced disastrous effects, the fall of pressure being exactly the same as before, but the relation of the higher to the lower pressure being now 4·4 to 1.

The method hitherto recommended for bringing men safely out of compressed air has been to decompress at a slow and uniform rate. But calculation on the principle already referred to shows that however slow this uniform decompression may be, the difference in partial pressure between the nitrogen dissolved in the tissues and the external air pressure will go on increasing during the decompression. Still more will the proportional difference between the two pressures increase. At the end of the decompression, therefore, the danger, if any, will be at a maximum; whereas during the first half, or more, of decompression time is simply being wasted, or much worse than wasted, if saturation of the body was incomplete, as is nearly always the case in diving. All our experiments confirmed this theoretical deduction.

These considerations led us to introduce a new method—that of decompression in graduated stages. The principle of this method is that the diver or worker in compressed air is brought rapidly to half the absolute pressure (or a little further if his tissues are not saturated), stopped there for a time, then decompressed a little further after sufficient time has elapsed to allow the maximum nitrogen pressure in his tissues to become not more than twice the nitrogen pressure of the air at the lower stage. He is then brought on by further stages on the same principle until he reaches atmospheric pressure. For the difference between the stages a pressure of 0·3 atmosphere was selected, corresponding to 10 ft. (or 3 metres) of sea water. The diver's progress is, of course, controlled by signal from surface, according to the indications of the pressure-gauge on the pump. The proper stoppages, after stays at different depths and for different periods of time, have been carefully calculated and put into the form of a table, which is now in use in the British Navy. A portion of this table is shown in Table V.

TABLE V

Portion of Diving Table for British Navy

Depth.		Total Pressure in Atmospheres.	Time from Surface to beginning of Ascent.	Depth and Duration in Minutes of Stoppages during Ascent.				Total Time for Ascent in Minutes.
Fathoms.	Metres.			12 Metres.	9 Metres.	6 Metres.	3 Metres.	
18–20	33–36½	4·6	Up to 15 min.	—	2	3	7	15
			15–25 „	—	5	5	10	23
			25–35[1] „	—	5	10	15	33
			35–60 „	5	10	15	25	57
			60–120 „	10	20	30	35	97
			Over 120 „	30	35	35	40	142

For ordinary diving work the table limits the stay on the bottom in such a way that the diver can come up safely within half an hour. This is desirable for many reasons: after prolonged stays on the bottom at great depths the time required for safe decompression, even by the stage method, is far too long; and with uniform decompression many hours would certainly be needed at the greatest depths.

[1] Ordinary limit of time.

The method of stage decompression was very thoroughly tested on goats, and compared with the old method of uniform decompression. The result was to show, we think, beyond all question that the stage decompression method is greatly superior, particularly for diving work. Even when the rate of decompression was much too fast by either method, stage decompression proved considerably safer. The accompanying table (Table VI.) shows a number of results obtained with the same animals and under the same conditions, except that the method of decompression was varied. It should be remarked that in each series, excepting C, the rate of stage decompression was less slow than what was calculated to be required for goats in order to prevent all symptoms, and considerably so in series B, D, and E. The occurrence of a few slight symptoms was therefore expected. It will be seen, however, that deaths and severe symptoms were entirely eliminated by the stage decompression, whereas a number of cases occurred with uniform decompression in the same time, besides a much greater number of slight cases.

TABLE VI

Absolute Pressure 6 *Atmospheres*

Series.	Exposure in Minutes.	Decompression in Minutes.	Number of Goats.	Stage Decompression.			Uniform Decompression.		
				Cases of "Bends."	Severe Symptoms.	Deaths.	Cases of "Bends.'	Severe Symptoms.	Deaths.
A	15	31	35	5	0	0	13	3	1
B	30	31	6	2	0	0	4	1	0
C	30	68	14	0	0	0	7	0	0
D	120	70	13	4	0	0	7	2	0
E	120	92	19	3	1	0	3	5	1
F	180	133	10	2	0	0	5	0	0
Total			97	16	1[1]	0	39	11[2]	2

With goats we found it necessary to provide for parts of the body taking as much as forty-five minutes to become half-saturated or half-desaturated with a given alteration of pressure. For man we therefore considered it necessary to provide for parts requiring seventy-five minutes, as the respiratory exchange per unit of body weight in man is three-fifths that of goats.

[1] Paralysis of foot lasting 1 hour.

[2] 3 cases of paraplegia, 4 of temporary paralysis, 2 of dyspnœa, and 2 of undefined illness.

A number of experiments in the steel chamber at pressures up to 6½ atmospheres were carried out on men, with stage decompression; and many dives in the sea to pressures of from 4 to

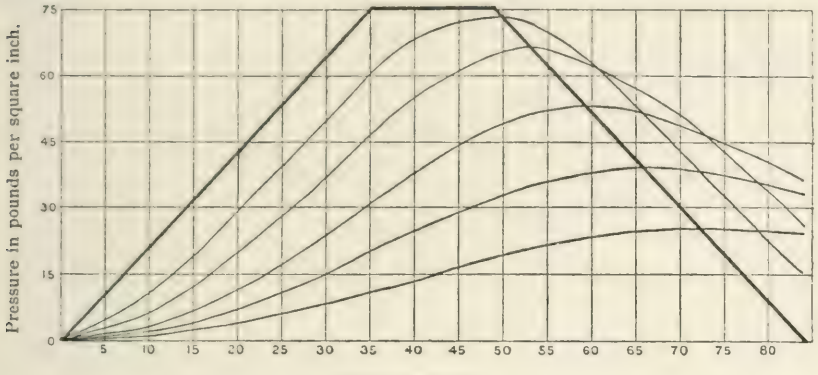


FIG. 2.—Diving to 28 fathoms by old method. The thick line represents the air-pressure. The curves, from above downwards, represent respectively the variations in saturation of parts which half saturate in 5, 10, 20, 40, and 75 minutes.

7⅓ atmospheres have also been made by Lieutenant Damant and others, with the time-limits and stage decompression recommended in the new diving tables. No symptom of caisson disease has, however, been observed, so that the new method

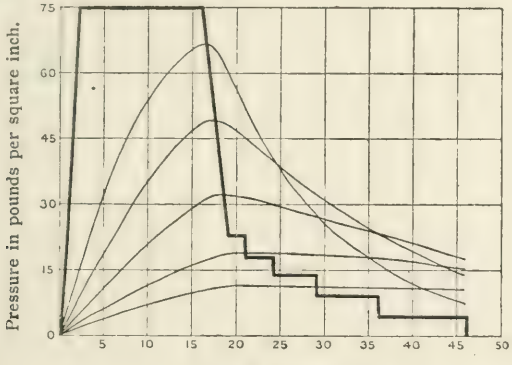


FIG. 3.—Diving to 28 fathoms by new method. The curves, from above downward, represent respectively the variations in saturation of parts of the body which half saturate in 5, 10, 20, 40, and 75 minutes. The thick line represents the air-pressure. Diver 14 minutes on the bottom and 46 minutes under water.

appears to be practically successful. The old method was to go down and come up slowly at a rate of about 5 ft., or 1½ metres per minute. The accompanying diagrams show the

calculated degree of saturation of different parts of the body during a dive to 6 atmospheres pressure for fourteen minutes with the old and the new method. It will be seen at once that the slow descent adds greatly to the danger, while during most of the uniform ascent many parts of the body are actually increasing in saturation; so that by the old method a diver runs a very serious risk of death or paralysis when he reaches surface. By the new method this risk is avoided, and the total time under water is reduced to nearly half. To obtain equally safe results by the method of uniform decompression it would, to judge from the experiments on goats, be necessary to extend the duration of decompression to several hours, the reason being that, in the case of short exposures to high pressure, not only is time wasted during the first half, or two-thirds, of the uniform decompression, but the saturation of most parts of the body continues to increase very seriously, so that at the end the decompression rate is far too fast unless the process is made very slow indeed.

After long exposures to compressed air, as in work in caissons and tunnels at moderate pressures, the saving in time by stage decompression is not so striking; but very substantial gains in convenience become possible. Time, however, forbids me from entering further into this subject, and I must now return to work in mines.

One of the most formidable difficulties met with in the deep mining which is now becoming so common is the great heat which is encountered. It is well known that the deeper we penetrate the earth's crust the higher is the natural temperature of the rock. The rate of increase is usually about 2·5° C. per 100 metres. At some places it is more than this: at others less. At Johannesburg, for instance, the increase is only 0·9° per 100 metres, while in Cornwall it is 3°. At a depth of 1,000 metres, which is now becoming common, the natural rock temperature is usually about equal to the body temperature. The air temperature in a mine may be either above or below the natural rock temperature. Fresh air direct from surface, or the evaporation of moisture, may cause a local reduction in mine temperature. On the other hand the slow oxidation of mineral substances, such as iron pyrites (FeS_2), very commonly causes the mine temperature to rise considerably above the original rock temperature. Miners are thus

much exposed to excessive heat, and the influence of this on their working powers and health is a matter of great practical importance.

In some of the mines of Cornwall the heat is probably as trying as in any place where men have to live and work; and my observations there led me to make experiments on a man's power of remaining in and working in warm air. It is of course well known that men can remain for a short time in extremely hot air, if it is dry. A temperature of 100° C. in a Turkish bath, or even a much higher temperature, can easily be borne for a few minutes; but there is surprisingly little information as to what temperature a man can continue to withstand for hours at a time during his ordinary occupation. My observations were made on several persons, partly in mines and partly in artificially heated rooms.[1]

One main result was that in still air, during rest, and with the subject of the experiment stripped to the waist, the maximum temperature which could be borne for several hours without marked pathological symptoms was 31° to 32° C. (88° to 90° F.) by the *wet-bulb* thermometer. Neither the actual temperature by the dry-bulb thermometer, nor the degree of humidity, nor the percentage of moisture in the air, mattered by itself. The determining factor was simply the wet-bulb temperature. If this exceeded 31° to 32° (the actual critical point seemed to vary slightly in different individuals), the body temperature began to rise steadily. The accompanying diagram shows the results of several experiments made on myself.

It was somewhat remarkable that when the air temperature was just above the critical point, the rise of body temperature did not slacken or cease as the difference between body temperature and wet-bulb air temperature began to increase. Actually the rate of rise of body temperature seemed to increase rather than diminish. The reason for this is the fact, clearly proved in some quite recent experiments by Dr. Sutton, of Oxford University, that the respiratory exchange, and consequently the heat production in the body, rises very markedly with even a slight increase in the rectal temperature. Hence the heat-loss cannot catch up the heat-production, and the body is in a "vicious circle."

[1] Details of part of these experiments will be found in the *Journal of Hygiene*, vol. v. p. 494.

A rise of 2° C. over the normal day body temperature was sufficient to produce much discomfort—a rapid pulse rate, and marked dyspnœa on exertion. A greater rise than this produced alarming symptoms when any considerable exertion was made, as in climbing ladders underground.

In a good air current the critical wet-bulb temperature was found to be about 2° C. higher. During work, on the other hand, the critical wet-bulb temperature was considerably lower—about 26° to 27° with fairly hard work in still air.

That the wet-bulb temperatures should be the determining

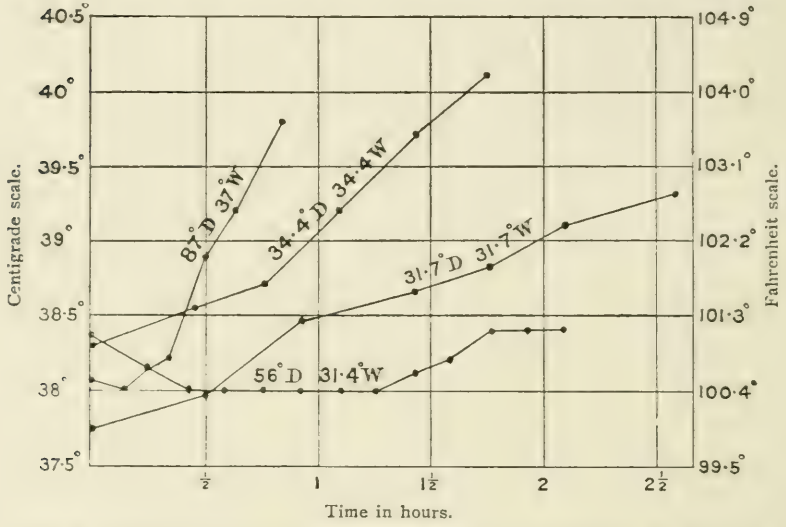


FIG. 4.—Variations of rectal temperature during rest, stripped to the waist, at various air-temperatures.

D = dry-bulb air-temperature. W = wet-bulb air-temperature.

factor is exactly what might have been predicted on physical grounds; and as the body, unlike the wet-bulb of a thermometer, is itself a source of heat, it is equally intelligible that moving air raises the critical wet-bulb temperature, and increased heat-production in the body lowers it.

A further point brought out strikingly in some of the experiments was that the sweating produced by heat depends primarily on the skin temperature, and not on the internal temperature of the body. There is quite commonly a fall in the rectal temperature at the time when most profuse sweating breaks out on going into warm air. On the other hand, sweating does not

occur in hot air until the skin temperature reaches a certain point. If the skin is cold to begin with it may be half an hour or more before sweating breaks out in a very warm room, whereas there will be profuse sweating within a minute if the skin is warm to start with.

I found that the Cornish miners were frequently employed in places where the wet-bulb temperature was from 30° to 38°. It might be supposed that, under such circumstances, cases of heat-apoplexy would be common; but I never could hear of any such cases, nor did I find, even in the hottest places, that the men allowed their temperatures to rise more than very slightly. What they did was simply to come out and rest in a cool place as soon as they felt that they were getting too hot. In fact, they seemed to be nearly always resting in this way. At a wet-bulb temperature of about 27° the amount of work done by a miner begins to fall off, and as the wet-bulb temperature rises further the work done gradually diminishes to a vanishing point. It may be that a miner's health sometimes suffers from the heat; but I could not find any clear evidence of this, and the men working in hot mines seem to be usually quite healthy. What undoubtedly suffers very heavily is the pocket of the mine-owner.

On the other hand, mine-horses often die of heat-apoplexy; and so fatal is the heat to horses that their employment is avoided in some of the hotter mines. They are liable to be worked too hard in a warm mine, just as soldiers are liable to be overmarched in hot weather; whereas the miners, who are perfectly free to follow the promptings of nature—for instance, to take off their clothes, stop working, or go into a cool place, or even into cool water, when they are too hot—escape without harm.

It seems clear that, for the economical working of a mine, the wet-bulb temperature should not in general be allowed to rise beyond about 27°—unless, perhaps, where there is a good air-current. To maintain this limit in the ordinary working places does not seem to be very difficult, provided the right measures are taken in laying out the ventilation. This matter will soon have to be studied much more closely by mining engineers than has been the case hitherto.

I should like, in conclusion, to point a moral, if I may, in connection with the hygienic regulation of engineering and

manufacturing work. No one doubts that the health, efficiency, and comfort of our working population are matters of the utmost importance. But the desirability of properly investigating the hygienic conditions of various industries is scarcely yet recognised as it ought to be. There is plenty of talk and a large number of regulations; but real knowledge—the indispensable basis of effective action—is too often lacking, and can be gained in no other way than by careful and detailed observation and experiment. This can be illustrated from all branches of hygiene—for instance, by the immensely important knowledge patiently gained during the last few years as to the nature and means of spread of various infective diseases.

I wish, however, to emphasise the same fact in connection with industrial hygiene; and perhaps the specific problems just discussed may enable me to do so. They concern the physical and chemical characters of the air in its relation to certain industrial employments. Insufficient study has, in spite of the best intentions, led to many mistakes in connection with these employments. First, as regards work in compressed air, we find on the one hand that divers have been more or less hampered or endangered by that somewhat rare condition, poisoning by carbon dioxide. But if we turn to men employed in compressed air in tunnels, etc., we also find the conditions—in England at least—reversed in a curious way. It has become a common popular belief that, provided the carbon dioxide percentage is low enough, all must be well with any kind of air. For instance, in the case of the large tunnel at present being constructed in compressed air under the Thames at Rotherhithe, London, the County Council, regardless of the enormous expense, has prescribed that, in view of the danger of caisson disease, the proportion of CO_2 shall not exceed that in the average air outside by more than 4 volumes per 10,000. It is, of course, not conceivable that by increasing the air supply to such an extent caisson disease can be prevented. The chief effect of the large air supply seems to be that the tunnel is very warm, as in consequence of the large quantity of air it is not effectively cooled between the compressors and the tunnel. The large air supply is thus probably doing more harm than good; and the absence of effective regulations for preventing caisson disease lays the County Council open to criticism in view of the long-existing knowledge of the subject.

Again, the well-intentioned regulation hitherto in force in the British, German, and other navies, that a diver should always descend very slowly, has probably cost many a diver his life.

To turn to the mining industry, one finds that often no expense is spared, and burdensome regulations may be imposed by law, in the endeavour to cure by unsuitable means evils for which a little real investigation would have shown an effective cure. In the Cornish mines, for instance, more ventilation was being provided as a remedy for the evils caused actually by stone dust and ankylostomiasis. The increased ventilation was apparently only aggravating the evil by making the working places drier and more dusty, just as in coal mines increased ventilation has, by itself, often increased the danger of colliery explosions by making the coal dust drier. Courrières Colliery, where 1,100 men were killed by a coal-dust explosion last year, was probably one of the best-ventilated mines in Europe.

As regards heat and moisture in the air, we have in England the curious anomaly in our Factory Act that the wet-bulb temperature in cotton factories, where women are employed all day, is expressly permitted by law to rise to even 37°, provided that the proportion of CO_2 does not exceed 9 volumes per 10,000! I do not for a moment mean to imply that any manufacturer would permit such a state of matters to exist in his factory, or that the workers would stay for an hour in such an impossible atmosphere; but the extemely well-meant English regulations as to temperature and humidity in the air of factories furnish one instance out of many, illustrating the need for more knowledge and investigation.

Such knowledge must be gained to a large extent on the spot, and not merely in a laboratory. Men who are tied to their work in a laboratory are often called upon to express opinions about matters of industrial hygiene without having any adequate opportunity of personally investigating the actual conditions. It is almost invariably the case, however, that investigations on the spot are essential, and that discussion with engineers, managers, and workmen throws entirely new light on the subject-matter. For one thing, the engineers and managers help us, beyond everything, to think of things quantitively and in their relations to other things. This lesson

is one which is often greatly needed in our branch of science. In spite of the inspiring examples of such men as Pettenrofer or Paul Bert or Parkes, we seem constantly in danger of forgetting it. We should progress hand in hand with the development of each industry, and be ever ready to help the engineer or manufacturer. It is only by doing this on the one hand, and by keeping in close and living touch with physiology and pathology on the other, that industrial hygiene can properly fulfil the very important duties which human progress has assigned to it.

THE GREAT SCANDINAVIAN OVERTHRUST

By J. W. GREGORY, F.R.S.

Professor of Geology, University of Glasgow

Scandinavia and Scotland are geologically nearly akin, and some of the difficult problems of Scottish geology may be solved by the Scandinavian evidence, which is often bolder and simpler.

The two lands were originally both part of the old Continent of Arctis; and they both have a similar foundation of crystalline schists and gneisses, which in Scandinavia are accepted as Archean. In both countries the older rocks are covered by wide sheets of pre-Cambrian sandstones—the Torridonian of Scotland and the Algonkian series of Scandinavia. Both countries have fossiliferous Cambrian rocks along their north-western edge, with a wider development of Lower Palæozoic, and even younger sediments to the south-east of the crystalline mass. That the schists of Scotland were once connected with those of Scandinavia is suggested by their frequent coincidence in strike, indicating that the structural grain of the countries was continuous across the North Sea. The geological connection has, however, often been geographically broken, and the direct land bridge apparently existed last in Pliocene times.

The student of crystalline schists in Scandinavia has the advantage over the Scotch geologist of the presence, in many localities, of fossiliferous Lower Palæozoic rocks in close association with the schists. In the Scottish Highlands, on the other hand, the fossiliferous rocks are restricted to a band along the north-western edge, except for a couple of localities containing doubtful worm tubes. The structure of Scotland, in its turn, helps to elucidate that of Scandinavia; for it was research on Scottish geology that explained the occurrence of fossiliferous sedimentary rocks beneath series of crystalline schists and gneisses. Murchison attributed this

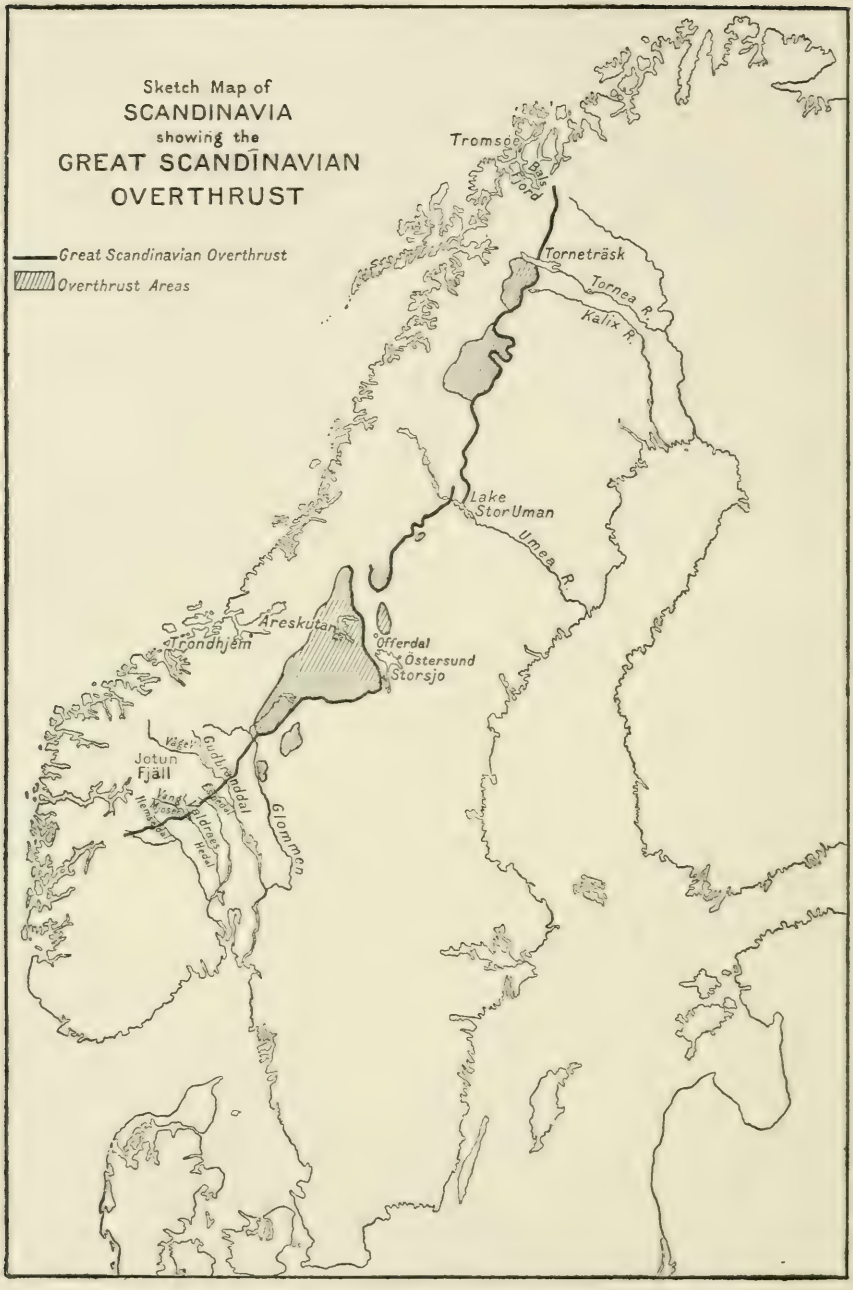


FIG. 1.

[The Valdraes is the valley marked above the name.]

arrangement, in the case of the Scottish Highlands, to selective metamorphism having converted the upper beds into schists, while leaving the lower beds unaltered. This view was also adopted in Scandinavia. It assumed, in accordance with the law of superposition, that the overlying beds must be the younger, and the chemical difficulties of selective metamorphism were left for subsequent explanation. That these difficulties were great was evident in Scandinavia from the observations made by Gumaelius in Offerdal during 1869. He then showed, in a section reproduced as fig. 2, the existence of a block of mica schist overlying phyllites or clay slates, with relations which selective metamorphism could hardly explain.[1]

Murchison's theory was overthrown in Scotland by the discovery that the crystalline schists had been thrust into positions where they appear to rest conformably on younger

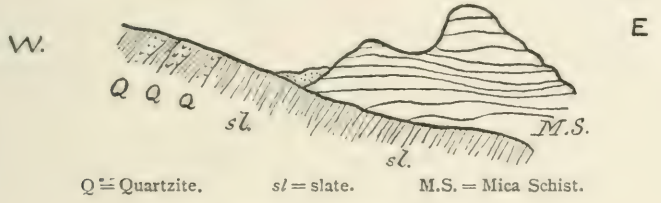


FIG. 2.—Section in Offerdal by Gumaelius.

beds. And the same explanation has been demonstrated in Scandinavia, although there it appeared at first almost incredible owing to the vast scale of the phenomena.

The literature of the Scandinavian overthrust is somewhat scattered, and it is mostly written in Swedish or Norwegian. Hence a brief statement of some conclusions and reference to the original authorities may be of value to British geologists. It has recently been my privilege to examine one of the typical overthrust sections on the shores of Lake Torneträsk in Swedish Lapland. For directions and kind help in this visit, and for showing me the material bearing on this question in the museums at Stockholm and Upsala, I owe my warmest thanks to Prof. Högbom of Upsala, Dr. Gunnar Anderson, Director of the Geological Survey of Sweden, and Dr. Holmquist, of the Technical High School of Stockholm. Nor must I omit mention

[1] This section was referred to by Törnebohm in 1873, *Sver. Geol. Unders. Ajhandl.* No. 9, p. 42; but it apparently remained in manuscript until it was published by Högbom in 1894.

of my indebtedness to the writings of Herr A. E. Törnebohm, late Director of the Geological Survey of Sweden, to whose work the present interpretation of the complex mountain structure of the High Fells of Scandinavia is so largely due.

Scandinavia consists geologically of a foundation of Archean and pre-Cambrian rocks, which are exposed over nearly all Eastern and Southern Sweden; they also form the fringe of islands along the Atlantic coast and most of the rugged southern provinces of Norway between Christiania and Bergen. The second important constituent is a band of schists and gneisses, which forms the mountain axis of Scandinavia, parallel to the Atlantic coast. It was doubtless once part of an old fold-mountain chain, which has now been cut down into a dissected pene-plane, wherein the existing ridges are independent of the heights formed by the original folds. The schists of this region are associated with thick masses of sandstone and conglomerate —the sparagmites and Dala Sandstone. Some of these rocks were once believed, as by Murchison, to be Old Red Sandstone; but they are now generally recognised as of pre-Cambrian age, and accepted as members of the Algonkian system. The schist belt also includes some fossiliferous rocks; and it has been held that the schists themselves are only Palæozoic sediments that have undergone thorough alteration. They were divided by Törnebohm in 1872 into two series, a lower or Seve Group, and an upper or Köli Group, and both were then regarded as Silurian or even post-Silurian.[1]

In addition to these rocks, which are doubtfully assigned to the Palæozoic, there are some slightly altered, sedimentary rocks, many of which are fossiliferous, so that their age is known. These rocks include representatives of the four oldest Palæozoic systems. Here and there from Lapland to south-western Scandinavia are areas of coarse sandstones, which are regarded as probably Devonian. The Lower Palæozoic rocks include members of the Silurian, Ordovician, and Cambrian; and their classification is somewhat complex, as they are developed with two very different facies—the eastern and the western.

The eastern facies includes normal sedimentary rocks, of which the upper members are shown by their fossils to include

[1] A. E. Törnebohm, "En geognostisk profil öfver den skandinaviska fjällryggen mellan Österund och Levanger," *Öfver K. Svens. Vet. Akad.* 1872; also *Sver. Geol. Unders. Ajhandl.* Ser. C, No. 6.

representatives of the Silurian and Ordovician systems; while beneath the fossiliferous rocks there are beds of alum shale and sandstone which, from their stratigraphical position, are referred to the Cambrian. These eastern rocks are but little altered, and they occur in simple regular sequence, with abundant limestones and fossils.

The Lower Palæozoic rocks of the western facies are lithologically strikingly unlike those assigned to the same age in districts farther to the east. The great bulk of the western rocks are schists; there are but few limestones, and fossils are found only in isolated deposits, which range in age from the Lower Cambrian to the Silurian. The striking lithological differences between the eastern and western rocks is explained as being partly due to more intense metamorphism in the western area; but this explanation is not alone adequate, as the rocks when first deposited must have been essentially different in composition in the two areas. It is therefore further explained that the "Silurian" rocks of the western area are due in part to extensive eruptions of basic rocks, while sediments were quietly accumulating in the seas to the east.

The Lower Palæozoic rocks of the western facies are themselves subdivided into the eastern and western sub-facies; their relations are somewhat uncertain, and the settlement of this question will be watched with much interest by Scotch geologists, from its analogy to some doubtful points in the correlation of the schists of the Southern Highlands of Scotland. Both the eastern and western sub-facies consist of series of phyllites, clay slates, schists, and gneisses, of which the most highly crystalline rocks belong to the lower division of the eastern sub-facies.

The rocks vary in metamorphism along their strike; though why the most highly metamorphosed areas are distributed as shown in Törnebohm's map is not obvious. Thus the Sul Schists extend from north-north-east to south-south-west in a narrow band 2 miles wide and 80 miles long; and it is marked as strongly metamorphosed for a length of about 22 miles; whereas the adjacent parallel band of the Meraker and Selbu Schists is strongly metamorphosed for 45 miles farther to the south-south-west. In both sub-facies there are some fossiliferous beds: namely, the Höilandets group of Trinucleus Sandstone age (Ordovician) in the western series, about 25 miles south-south-west from Trondhjem; and the Sul slates, containing

graptolites belonging to the lower part of the true Silurian, in the eastern series. These fossiliferous beds are held to support the view that the schists of the area are altered Lower Palæozoic rocks. This, however, is the one conclusion in Scandinavian geology which foreign geologists may be most excused if they hesitate to accept, until further evidence has been published that the fossiliferous beds cannot be younger rocks folded or faulted in among older schists. The solution which Prof. Bonney gave of the same problem in the Lepontine Alps may be applicable to the schists of the Trondhjem district.

The belief that these schists were all Silurian was, however, naturally adopted at first, because unaltered fossiliferous Lower Palæozic rocks are found in places beneath wide sheets of schist and gneiss. It therefore appeared necessary to accept the upper beds as the younger in age. But it has been proved that, as in Scotland and the Alps, the superposition of the schists is due to earth movements. And in Scandinavia the overthrusting has taken place on a scale greater than has been established in any other part of the world.

The development of these views may be seen from the successive explanations of the geology of Åreskutan in Jemtland, one of the best known Swedish mountains (4,645 ft.). The lower eastern and western slopes of this mountain consist of the normal Lower Palæozoic rocks of the eastern facies; they include the Cambrian alum shales, above which are more shales, and the Pentamerus Limestone of the base of the true Silurian. The whole of the upper part of the mountain consists of crystalline schists of the Seve Group; they include hornblende schist, mica schist, crystalline limestone, and garnetiferous gneiss. The base of the crystalline schists is a quartzite or quartz-schist, which is in places a conglomerate. "The contact between this quartzite and the Silurian clay rocks I saw laid bare," says Törnebohm,[1] "in a small stream that descends from the western side of Åreskutan. The dip is here not more than 15° or 20° to the east, and the superposition is very clear. The boundary between both formations stands out very definitely, yet without any marked discordance. Over the quartzite, which is not very thick, follow first hornblende schists, which alternate with mica and quartzitic schists, and include some small layers, only a few inches thick, of white crystalline limestone; further up come

[1] Törnebohm, *Afhandl. Sver. Geol. Unders.* No. 9, 1873, p. 41.

a somewhat thick quartzite schist, wherein occur the copper ores of Åreskutan; and then follow gneiss-like rocks (which sometimes include hornblende and sometimes garnets as accessory constituents), and they, with alternations of hornblende and mica schists, extend up to the summit of the mountain."

The same sequence is exposed on the eastern side of the mountain, and Törnebohm therefore naturally concluded that "the schists of the Seve Group, in spite of their crystalline structure and their primæval aspect, are younger than the surrounding Silurian deposits, and that between them occurs a definite geological boundary which is sometimes marked by beds of conglomerate."[1]

This conclusion is represented in a section across Åreskutan which is reproduced as fig. 3. Ten years later Törnebohm recognised that the superposition could not be trusted, as the relations

FIG. 3.—Section through Åreskutan after Torneböhm, 1872, representing the Are schists as post-Silurian.

of the Dala Sandstone (now recognised as Algonkian) had been in places inverted by overthrusting; but he still accepted the view that the Seve Group, with the Vemdal quartzite which he regarded as its base, is later than the underlying Lower Palæozoic rocks, and supported this conclusion by fresh evidence from the district of Herjedalen.[2]

The demonstration by Prof. Lapworth and by Dr. Horn, Dr. Peach and their colleagues of the Scottish Geological Survey, that the superposition of schists over normal sediments in the north-west Highlands of Scotland was due to overthrusting, encouraged Törnebohm to believe that the same process was the cause of the perplexities of mountain structure in

[1] Törnebohm, *Afhandl. Sver. Geol. Unders.* No. 9, 1873, p. 42.

[2] A. E. Törnebohm, "Om Vemdalsqvartsiten och öfriga qvartzitiska bildningar i Sveriges sydliga fjälltrakter," *Geol. För. Förh.* vol. vi. (1883), pp. 274–94. pl. xii. and xiii.

Scandinavia. In 1888 he redescribed the geology of Åreskutan,[1] and identified the schists and gneisses of the summit of the mountain as pre-Silurian rocks, pushed from the west over the lower but younger Palæozoic sediments. This view is represented in fig. 4 from Törnebohm's memoir of 1896.

In 1891 Törnebohm explained other cases of the apparent interstratification of slates and quartzites with gneisses as due to inversion by folding. Thus on the northern foot of the Jötun-Fjäll in Norway, on the southern shores of Lake Våge, the apparent sequence consists of the following beds, all of which are dipping southwards: (5) upper gneiss, (4) upper quartzite, (3) clay slate, (2) lower quartzite, (1) lower gneiss. Törnebohm explained this arrangement as due to an isoclinal, by which the quartzite and gneiss are repeated on

FIG. 4.—Section through Åreskutan after Torneböhm, 1896, showing the Åre schists as pre-Silurian.

both sides of the slates.[2] According to this interpretation there are only three beds, and the gneiss is the oldest.

This conclusion was, however, rejected by Björlykke, who in 1894 accepted the fact that much of the so-called Mountain Quartzite (*Höifjeldskvartsen*) of the Norwegian Fells was a gneiss and not a quartzite; and he claimed that this gneiss was of post-Ordovician age, and that the crystalline rock had been originally deposited unconformably on the unaltered, older Ordovician beds.[3]

In 1905 Björlykke issued as No. 39 of the *Norges Geologiska Undersögelse* a work on the mountain structure of Central Norway, in which he maintains that the upper sparagmite group is of Devonian age, and that, for that area, "the theory of overthrusting must be abandoned." For the geology of

[1] A. E. Törnebohm, "Om fjällproblemet," *Geol. För. Förh.* vol. x. (1888), pp. 328–36.

[2] A. E. Törnebohm, "Om högfjällsqvartsiten," *Geol. För. Förh.* vol. xiii. (1891), p. 41.

[3] K. O. Björlykke, "Höifjeldskvartsens nordostligste udbredelse," *Norges Geo.. Undersög.* No. 14; Aarbog for 1892 og 1893 (1894) pp. 64, 73, 74.

Mount Åreskutan he apparently accepts the suggestion of Reusch, that its upper gneisses are altered eruptive rocks. From the same memoir it appears that Schiötz, in 1902, accepted the action of overthrusts to explain the structure of Österdal in Eastern Norway.

In 1894 Herr Törnebohm's views received the powerful support of Prof. Högbom, now of the University of Upsala, who demonstrated the truth of the overthrust theory by careful mapping of the High Fells of Jemtland. His results were published in a monograph,[1] and illustrated by a geological map of the province on the scale of 1 to 500,000. His survey proved that the overthrusting has taken place on a scale even greater than in Scotland. Two years later followed Dr. Törnebohm's important memoir,[2] which described the geological structure of the main High Fells both of Norway and Sweden, and extended the overthrust explanation to the districts to the south-west of Jemtland.

The surveys of Törnebohm and Högbom leave no doubt as to the occurrence of a great overthrust of pre-Cambrian schists over Lower Palæozoic sediments. The main overthrust mass covers an area of some 5,200 square miles; this vast sheet of rock was thrust from west to east over the underlying fossiliferous sediments. The evidence is clearest in the case of some outliers of the schists, which have been cut off by denudation from the main overthrust mass. The best known of these outliers are at Offerdal, a locality to the north-west of Östersund and thirty miles east of Åreskutan. Offerdal is a valley running from north-west to south-east and containing the upper arm of Lake Nälden. The floor of the valley is occupied by the true Silurian beds, which are thrown into many small folds, and are often isoclinals dipping to the west (fig. 5). The valley is bounded to the north by a plateau, which is cut through by the valley of Lake Landösjön; the plateau and the area to the north of that lake are formed by overthrust mica schists and crushed Algonkian conglomerates and

[1] A. G. Högbom, "Geologisk Beskrifning öfver Jemtlands Län," *Sver. Geol. Unders. Afhandl.* Ser. C, No. 140 (1894), 107, p. 1, pl. and map.

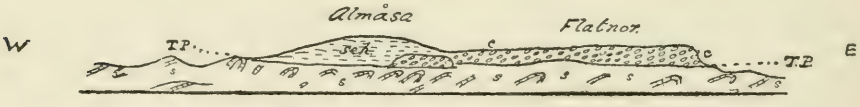
[2] A. E. Törnebohm, "Grunddragen af det Centrala Skandinaviens Bergbyggnad," *Handl. K. Svens. Vet. Akad.* vol. xxviii. No. 5 (1896), 210 pp. 4 pl. and geological map of Central Scandinavia, on scale of 1 to 800,000 (with German Summary pp. 179–97).

quartzites. The southern side of the Offerdal valley is formed by a block mountain known as Alsen, which is built of the same rocks.

According to Prof. Högbom's map, the Pentamerus Limestone (*i.e.* Silurian of the Llandovery Series) can be traced all round the foot of the Alsen outlier; and looking across Offerdal to the north, the superposition of the schists above the limestones is shown in a section, reproduced as fig. 5.

The maps and sections of both Törnebohm and Högbom appear convincing that the only satisfactory explanation of the facts is that the schists have been overthrust above the Lower Palæozoic sediments.

From Jemtland, the overthrust has been traced south-west into Norway and northward into Lapland. The evidence in the far north has been collected by several observers, including



s Contorted Silurian (chiefly Upper Silurian). *c* = Crushed Sparagmite.
sch Schists. (Seve Group.) T.P. = Thrust Plane.

FIG. 5.—The north bank of Offerdal, after Högbom.

Törnebohm, Högbom, Holmquist, Sjögren, and Svenonius.[1] Prof. Sjögren[2] has described the sections around Sulitelma, and Dr. Holmquist[3] the wide area of schists thrust over sediments assigned to the Silurian, to the south-east of Sulitelma and south-west of Qvikkjokk. To Dr. Holmquist[4] is also due the most detailed account of the overthrust area to the south of Torneträsk, the most northern of the great Swedish lakes. The evidence at Torneträsk is very clear, and, by the kindness of Herr H. Lundbohm of Kiruna and the Traffic Manager of

[1] A bibliography has been recently given by Högbom, Nörrland, 1907, pp. 108–9.

[2] H. Sjögren, "Om Sulitelma-områdets bergarter och tektonik," *Geol. För. Förh.* vol. xviii. (1896), p. 346.

[3] P. J. Holmquist, "En geologisk profil öfver Fjellområdena emellan Kvikkjokk och Norska Kusten," *Sver. Geol. Unders. Afhandl.* Ser. C, No. 185 (1900), 100 pp. 2 pl.

[4] P. J. Holmquist, "En geologisk profil öfver den skandinaviska fjällkedjan vid Torneträsk," *Geol. För. Förh.* vol. xxv. (1903), pp. 27–78, pl. i.–iii. And "Bihang till Törneträskprofilen," *ibid.* pp. 373–90, pl. xii.

the Lapland Railway, I was enabled to spend a few hours at Kaisepakte, where the thrust plane is most accessible.[1]

A bluff of rock here rises steeply above the southern shore of the lake; the lower part of the slope consists of a massive pink granite. Then follows a series of brown nodular shales, with some layers of conglomerate; next a green shale and a brown nodular shale with some concretions; then follows a bed of dark blue massive quartzite. The shales are normal sedimentary rocks resembling in aspect some of our Tarannon shales. A fossil has been found in them at Luopahta, a few miles farther to the east. Holmquist[2] quotes an opinion based on it that the beds are of the age of the Chasmops Limestone (Middle Ordovician); but the fossil appears to be indeterminable, though its presence encourages the hope that better material will reward further search. Above these Lower Palæozoic sediments comes a sheet of cataclastic schists, some of which still retain the characters of augen-gneiss; but the main mass of rock has been broken down into a mylonite. This shattered rock has been named "Kakarit" by Svenonius. All stages of mylonitisation may be seen, including crushed gneiss, banded felspathic quartzite, and banded granulites; the extreme stage in the dynamo-metamorphism is reached in some fine-grained pseudo-phyllites, which look like simple slates.

The overthrust rocks are intensely crushed and contorted, great blocks being composed of layers twisted into complex folds and overfolds. The underlying sediments, on the other hand, often remain horizontal or with a dip of only 2°. Examination of this section is convincing that the crystalline rocks that form the bluff of Kaisepakte have been thrust over the unaltered, undisturbed underlying sediments. The descriptions by Dr. Holmquist of the adjacent districts to the south of Torneträsk; by Törnebohm[3] of the country to the north of the lake at the head of Balsfjord, to the south-east of Tromsöe; by Sjögren of Sulitelma; and by Holmquist of the country south of Sulitelma, show that this overthrust traverses the whole length of Norrbotten, the northernmost province of Sweden.

[1] During my visit I had the benefit of the company of Herr Ljüstrom and Herr Sundius of Upsala.

[2] Holmquist, *Geol. För. Förh.* vol. xxv. p. 38.

[3] A. E. Torneböhm, "Om formationsgrupperna inom det nordligaste Skandinavien," *Geol. För. Förh.* vol. xxiii. pp. 206–17.

There is also evidence of the overthrusting across Westerbotten, between Norrbotten and Jemtland. The overthrust can indeed be traced from 69° N. lat. in Norwegian Lapland across seven degrees of latitude in Sweden, till the line recrosses the frontier and continues for 150 miles farther into Norway. Its course in Southern Norway was traced by Törnebohm across the valleys of the Glommen, of Gudbrandsdal, Espedal, the Valdres valley (Hedal), and Hemsedal. His conclusions were supported by Reusch's[1] description of the country around Vangs Mjösen and Slidre, in the upper part of the Valdres Valley, where the Archean granites are overthrust over phyllites and quartzites assigned to the Cambro-Silurian. And the course of the great overthrust has been continued westward round the Sognefjällen to the Hardangerfiord, by the work of Reusch, Rekstad, and Björlykke,[2] and by Rekstad in the Hemsedalfjällen.[3]

The Scandinavian overthrust is therefore on a colossal scale. It follows the main mountain chain of Scandinavia throughout its entire length. Its length, according to Törnebohm, is about 1,800 kilometres (over 1,100 miles). It is therefore on a much larger scale than in Scotland, where the length of the overthrust from Glenelg to its northern end at Whitten Head is about 100 miles. The width of the overthrust is even more startling. Törnebohm claims that the overthrust masses, which are still 1,500 metres thick, have travelled eastward for a distance of eighty miles. He remarks that "such an immense width is something never yet precedented in the annals of geology, and he does not conceal from himself the fact that its possibility will be generally doubted. But after many years of investigations he has come to the conviction that only by such a great overthrusting can the actual relations be explained; so he is not afraid to express this conviction boldly." Törnebohm claims that the width of the overthrust is not disproportionate to its length. The demand made on the strength of the rocks is, however, at first staggering; so it is not surprising that the development of the overthrust hypothesis in Scandinavia met with some opposition.

[1] Hans Reusch, "Höifjeldet mellem Vangsmjösen og Tisleia (Valdres)," *Norges Geol. Undersög.* No. 32; Aarbog for 1900, pp. 45–88.

[2] "Fra Hardangervidden," *Norges Geol. Undersög.* Aarbog for 1902, No. 2.

[3] *Ibid.* 1903. The south-western extension of the overthrust has been described by Törnebohm, "Om den skandinaviska fjällkedjans sydvestände," *Geol. För. Förh.* vol. xxv. (1903), pp. 282–8.

The criticisms have followed three main lines. (1) The post-Silurian age of the schists and gneisses overlying the sediments is still maintained by several Scandinavian geologists. (2) The superposition of the older beds has been attributed by Svenonius to complex overfolding; he explained the succession of the rocks in Norrbotten by contortions similar to those formerly introduced in Swiss geology (fig. 6, after Svenonius). (3) The invocation of numerous overthrusts instead of one vast movement.

The third hypothesis has been advanced by Dr. Holmquist, from the evidence collected in his careful study of the districts to the south of Torneträsk. He holds that instead of there having been one vast overthrust, which in that area would have to be thirty-five miles or more in width, there have been many shorter overthrusts. The "Silurian" rocks have been thus sandwiched between the crystalline rocks; and,

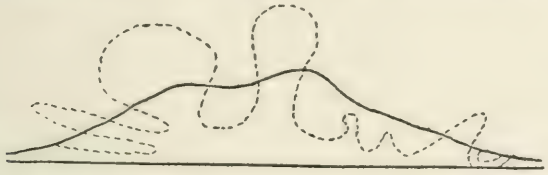


FIG. 6.—Section through Daunevarra, Lapland (after Svenonius, 1896).

according to Dr. Holmquist, the tops of the folds sometimes expand laterally beyond their impressed limbs, like an exaggerated Alpine fan. But these sections have been ridiculed as "mushroom constructions" by Törnebohm, who holds to his faith in one great movement.[2] Dr. Holmquist's view, however, appears to be possible, and it has the recommendation of making less trying demands on the strength of the rocks. The overthrust rock-masses are comparatively thin in proportion to their extent, and it would appear very doubtful whether they could have been thrust forward in a slab by pressure applied thirty miles behind in the case of Lapland and eighty miles distant in Jemtland. We are forbidden to appeal

[1] F. Svenonius, "Om Berggrunden i Norrbottens Län," *Sver. Geol. Undersok. Ajhandl.* Ser. C, No. 126, 1892, pp. 37–41, and "Några Bidrag till belysning af eruptivens betydelse för fjällbildningarna," *Sver. Geol. Unders. Afhandl.* Ser. C, No. 161 (1896), pl. 1.

[2] A. E. Törnebohm, "Om Torneträsk-profilenstydning," *Geol. För. Förh.* vol. xxv. (1903), pp. 83–92.

to a very thick overlying rock cover, or the underlying shales must have been more disturbed. The process must have happened in the fracture zone of the crust and not in the deeper zone of fluxion. Hence if Dr. Holmquist's view be found correct, and could be extended by further mapping to Jemtland, it would simplify the explanation of the crustal movements, and would still leave the Scandinavian overthrust the greatest yet discovered.

The Scottish and Scandinavian overthrusts are accompanied by many of the same phenomena. The most significant difference is that the movements took place in opposite directions; in Scotland the rocks above the thrust planes were pushed westward; in Scandinavia they travelled eastward. The disturbances were therefore doubtless due to changes in the land that occupied the area between the Scandinavian High Fells and the North-West Highlands of Scotland. The thrusts, on both sides, were outwards towards the depressions that probably existed in the Baltic region and off the western coast of Scotland.

Supplementary Note on the Literature

In addition to the literature cited in the text reference may be made to the following. Full bibliographies of the earlier literature are given by Törnebohm in his *Grunddragen*, 1896, pp. 202–5, and by Högbom in his Monograph on Jemtland, 1894, pp. 100–3.

A short summary of the literature on the investigations of 1868–71 is given by Törnebohm, "Till historiken öfver de geologiska undersökningarna i Sveriges fjälltrakter," *Geol. Fören. Förhandl.* vol. vii. 1885, pp. 669–73.

A summary is given by Prof. Högbom in the third section of his paper, "Sur la tectonique et l'Orographie de la Scandinavie," *Annales Géogr.* vol. xi. 1902, pp. 122–33, pl. v.

The course of the overthrust line in Sweden is marked on the geological map of Sweden on the scale of 1 to 1,500,000, issued by the Swedish Geological Survey in 1901.

Many of the memoirs quoted from the *Afhandlingar* of the Geological Survey of Sweden were also published in the *Förhandlingar* of the Geological Society of Stockholm.

THE REPAIR OF A NERVE

By W. D. HALLIBURTON, M.D., F.R.S.

It is not probable that many readers of Science Progress have ever experienced the misfortune of having an important nerve cut across. But those engaged in hospital practice, especially in manufacturing districts where machinery is employed, often see accidents of this nature. The result of such an injury is that the parts of the body which the divided nerve served to link with the central nervous system are destitute of the power to move and to feel. The telegraphic service of the body has been interfered with, and the brain is no longer able to transmit its commands to the motor mechanism, nor to receive communications from that part of the sensory surface which is severed from it by the division of nerve fibres. Many of these cases recover after the lapse of time, and this period can always be shortened if by surgical interference the divided segments of the nerve are sutured together.

If, on the other hand, the injury occurs in the central nervous system, the effects are more serious. Suppose, for example, the spinal cord is divided or crushed, then the main line of communication is no longer capable of conducting nerve impulses up and down, and a large area of the body surface is thrown out of connection with the sensorium. And again, unless the division is a complete one, a certain amount of recovery takes place. But the method of repair is different in this case from what it is in our first example—the division of a nerve.

In the recovery that occurs to a small extent when important conducting paths are cut across in the central nervous system, restoration is not due to the healing of the injured tracts, nor to the regeneration of the divided threads, which are called nerve-fibres; but to the happy circumstance that the complex labyrinth of conducting paths has so many cross connections that it is possible for impulses to avail themselves of these,

and so reach their destination by a new channel. There is absolutely no recovery if the division of the spinal cord is complete, for then all possible channels are equally blocked.

In a nerve, recovery will occur if the division is absolute, and in this case restoration of function is due to the regeneration of the nerve fibres. It is the object of this paper to describe the manner in which this regeneration occurs.

Nearly every one has suffered at some time from minor injuries in the way of skin wounds, and has doubtless for a time experienced anæsthesia or loss of sensation on one side or other of the cut. Some months later sensation returns, and the accident is forgotten. Regeneration of the nerves necessary for this usually occurs without any surgical aid, and and all that remains is a scar. Scar tissue, or cicatricial tissue, as it is technically termed, is a dense kind of connective tissue somewhat stronger and firmer than the ordinary skin. This same kind of tissue is formed in the repair of all injured structures in the body. When, for instance, a muscle or a tendon is cut across, the formation of cicatricial tissue to bridge across the gap is quite sufficient to restore the continuity and uses of the injured organ. If a rope is divided, it does not matter what sort of material—silk, hemp, or cotton—is used to tie the ends together; if the repairing material is sufficiently strong the rope is as good as ever. So it is with the haulage mechanism of the body: a tendon or a muscle can pull upon the bones quite as well as before the accident, even although the repairing tissue is not identical with that which was injured.

But the case of a nerve is different. If a telegraph wire is divided the repairing material must be wire, or at least something capable of conducting electricity. The molecular change transmitted along nerve fibres, and which is termed a nervous impulse, is so peculiarly the property of nerve fibres, that no amount of scar tissue is efficacious in restoring function. Nerve fibres must be mended by nerve fibres and by nothing else. Indeed, a large amount of cicatricial tissue is harmful, for its very density is an obstacle to the successful growth of the new fibres which ultimately penetrate it.

We must never push the telegraphic simile too far in dealing with the nervous system, useful as it is in talking of such matters to students. If a telegraph wire is broken, all that is necessary in healing the damage is to connect the ends

with another piece of similar wire; the two segments of wire which are united are not injured throughout their length, and conduct the electrical impulse as well as ever. In the case of a nerve it is different; the stretch of nerve still united to the central nervous system is spoken of as the central end; this consists of long threads which are the outgrowths of protoplasmic masses, termed nerve cells, situated in the grey matter of the central nervous system. The central end of a divided nerve remains healthy, or approximately so. The other segment, which is called the peripheral end, consists of nerve fibres also, but after division they soon lose their power of responding to stimuli and of conducting nerve impulses. They simultaneously undergo a degenerative change, which can be easily recognised by the microscope, and ultimately atrophy. These fibres having been cut off from the nerve cells—of which they are outgrowths—are no longer capable of receiving that nutritive control which it is the special object of these protoplasmic masses to accomplish. This was first pointed out by the elder Waller, and the term "Wallerian degeneration," by which it is known, is a permanent record of this historical fact.

Even the most rapid suturing of the divided ends together is ineffective to prevent Wallerian degeneration from occurring in the distal segment.

This shows the difficulties attendant on the process of nerve regeneration; for the new fibres have not only to bridge a gap, but they are also necessary to replace the degenerated fibres in the peripheral segment right down to its ultimate and final ramifications in skin, muscles, or other organs.

In the elucidation of the problem as to how this is accomplished, experiments upon animals have been rendered necessary, so as to extend and complete the imperfect information, which is all that can be obtained by clinical observation in man.

But in order to pave the way to this, it is first necessary to allude to the way in which nerve fibres find their way to the periphery from the central nervous system in the embryo. The central nervous system (brain and spinal cord) is originally formed by an infolding from the surface of the most external embryonic layer known as the epiblast, the remainder of the epiblast appearing in the fully formed animal as the epidermis. This infolding is cut off from the surface

epiblast, and an island tube of epiblast is converted by thickening of its walls into the brain in front and spinal cord behind. Its walls contain numerous masses of protoplasm, or primitive nerve cells, which are called neuroblasts. At first these cells have no processes, but branching projections grow out from each; these interlace with the similar processes of other cells, and so bring neighbouring cells, and even distant cells, into communication with each other, to ensure harmonious or co-ordinate action between them. But these branching processes or dendrons are not the only outgrowths of the nerve cell, nor are they the first to make their appearance. The first outgrowth to appear is stouter than those that follow; it grows to the greater distance; it leaves the central nervous system altogether, and ultimately penetrates to peripheral structures. It is known as the axon, and, with few exceptions, every cell body has only one axon. It ultimately becomes the axial conducting core of a nerve fibre, and a collection of these fibres constitutes the bundle known as a nerve. At first the axon is naked throughout its entire length, but later in development it becomes sheathed. The sheaths of the axon are certainly important for its nutrition, and possibly insulate the axon, much as a telegraph wire is insulated from its fellows by a silk or gutta-percha covering. In the fully formed nerve each fibre then consists of an axon or axis cylinder; outside of this is a thick white sheath, which contains complex phosphorised fats—this is known as the medullary sheath or white substance of Schwann; outside of this is a delicate nucleated membrane called the primitive sheath of Schwann, or more usually the neurilemma. Certain new fibres, however, never receive a fatty sheath, and are consequently called non-medullated; these are the fibres which supply the involuntary muscular fibres of the internal organs of the body.

This view, that each nerve fibre develops as an independent outgrowth from a nerve cell, and grows distalwards, finally becoming united to other tissues (*e.g.* muscle fibres) in the periphery of the body, is associated especially with the name of His, and has been accepted by the majority of embryologists.

There have been other views held, but it will be sufficient here to mention only one of these; for it is the one which, next to that of His, has been favoured by investigators. Briefly it is as follows: The nerve fibre is not a secondarily formed

bridge between the central nervous system and the peripheral organs, but exists from the very first, and in subsequent development it merely undergoes elaboration, and increases in bulk and in length as the distance from the central nervous system and the periphery increases with the increasing size of the developing animal.

I shall not fully discuss the pros and cons of this controversy, but only say that the available evidence appears to me strongly in favour of the first of the two views, and it has within the last few months been supported by a very remarkable ocular demonstration of its truth. Mr. Ross Harrison has, in the Johns Hopkins University, Baltimore, actually seen the fibres growing outwards in embryonic structures. Pieces of the primitive nervous tube which forms the central nervous system were removed from frog embryos, and kept alive in a drop of lymph for a considerable time; the cilia of the neighbouring epidermic cells remained active for a week or more; embryonic mesoblastic cells in the vicinity were seen to become transformed into striated muscular fibres, and there was therefore no doubt that even under artificial conditions of this kind—rendered necessary for microscopic purposes—life and growth were continuing. From the primitive nervous tissue, and from this alone, nerve fibres were observed growing and extending into the surrounding parts. Each fibre shows faint fibrillation, but its most remarkable feature is its enlarged end, which exhibits a continual change of form. This amœboid movement is very active, and it results in drawing out and lengthening the fibre to which it is attached, and the length of the fibre increases at the rate of about one micro-millimetre in one or two minutes.

I think these observations show beyond question that the nerve fibre develops by the overflowing of protoplasm from the central cells, and thus give us direct evidence in favour of the view which most embryologists previously held mainly as the result of circumstantial evidence. Such, then, being the general state of our knowledge regarding the way in which nerve fibres grow in the developing animal, it is not surprising to find that the prevalent idea regarding their regeneration after injury follows the same lines. The original teaching of the elder Waller (1852) that regeneration occurs by fibres growing out from the central stump into the peripheral segment of the nerve

was formulated at a time when the relationship of nerve fibres to nerve cells was not so fully recognised as it is at present; and the Wallerian doctrine may be accepted with confidence to-day. It has, however, been questioned from time to time, and the earliest to hold an opposite view was Vulpian. Vulpian, working with Philippeaux, cut nerves in young animals, excising long portions so as to prevent the two ends uniting. Some months later they were surprised to find that new perfectly formed nerve fibres had appeared in the peripheral segment, and that this segment possessed the physiological properties of being excitable and capable of conducting nerve impulses. To this phenomenon they gave the name of "autogenetic regeneration." The publication of these results provoked a long controversy, which lasted from 1859 to 1874, and was closed at the latter date by Vulpian withdrawing his new idea. He did so because in the meanwhile he had repeated his experiments more carefully, and so discovered that, although the ends of the divided nerve had not joined up, connection with the central nervous system had nevertheless been re-established by means of fibres growing into the peripheral segment from other nerves cut through in skin and muscle in the course of the operation.

The controversy has been revived within the last few years, and the position of the disputants has been almost exactly the same as that occupied by Waller and Vulpian half a century ago. Modern investigators have, however, the advantage of being able to apply new methods of research, and are provided with many histological reagents of which the older workers were destitute. It is, however, never safe to argue entirely from microscopic appearances, for nerve fibres may be simulated by non-nervous structures, and a strand that looks like a nerve fibre is not really such unless it can be experimentally shown to be both excitable and capable of conducting nerve impulses.

Vulpian's old doctrine of auto-regeneration has been revived in this country by Ballance and Purves Stewart, and in Scotland by Kennedy. The most prominent and persistent supporter of the autogenetic theory is, however, a German neurologist named Bethe. But none of these investigators have excluded the fallacy which underlay the work of Vulpian and Philippeaux, as has been recently pointed out by Langley and Anderson. These two workers at first thought they also had obtained evidence of purely peripheral regeneration, and it was not until

they carried out careful dissections that they convinced themselves that union with the central nervous system had occurred. The new nerve fibres which grow into the peripheral segment from other nerves divided in the operation, often do so by a devious and contorted course. If the number of medullated nerve fibres in the peripheral end is small, then the connection with central fibres was found to be slight; and in cases where no connection occurred then medullated nerve fibres were entirely absent. Bethe admits a variability in the number of medullated fibres, and this, though easily explicable on the view that such fibres come by accident from the central ends of divided nerves, is not accounted for at all by the autogenetic theory.

Another admission which Bethe makes also weakens his view. He admits that auto-regeneration does not occur in adult animals. He does not, however, state at what age animals lose their power of auto-regeneration, nor explain how it is that after a certain date an animal repairs a divided nerve in the exactly contrary manner to that it would have adopted if the nerve had been cut before that date. The difference which exists between young and adult animals is quite easily accounted for in accordance with Wallerian views; it is simply due to the greater ease with which reunion with central nerves occurs in young and actively growing animals.

Bethe's views have been contested not only by Langley and Anderson, but also by Lugaro, by Kölliker, by Cajal, by Marinesco, by Mott, and Edmunds in conjunction with myself, and by numerous others.

I may mention a few of the experimental results which have come out of the renewed work elicited by the promulgation of the autogenetic theory.

(1) It is possible entirely to prevent reunion with the central ends of divided nerves. In our own work we accomplished this by removing a long stretch of the main nerve experimented with, by making the skin incision as small as possible, and by inclosing the top end of the peripheral segment in a cap of sterilised gutta-percha. Under such circumstances no auto-regeneration occurs.

(2) Pieces of nerve may be transplanted under the skin, and in time a few fully formed medullated fibres appear within the degenerated bundle of fibres. This is adduced by Kennedy as undoubted evidence of auto-genesis, but, again, is easily

explicable on the hypothesis that the new fibres had wandered in from cutaneous nerves divided in the course of the operation, and we showed that if this fallacy is excluded by transplanting the nerve, not into the subcutaneous tissues, but on to the stomach wall in a sheath of peritoneum, no regeneration occurs at all.

(3) The late appearance of the medullary sheath in those portions of the regenerating fibres which are most distant from the place where the nerve is originally cut and sutured, is a conclusive piece of evidence that the new nerve fibres grew from the central end in a peripheral direction.

(4) After regeneration has occurred, the nerve may be again cut across, either on the central side of the original point of section (as in Langley and Anderson's work), or on the peripheral side of the original seat of operation (as in our own work). In the former case Wallerian degeneration occurs in all the new fibres, showing that they were all under the nutritive control of the cells of the central nervous system. In the latter case the degeneration took place solely on the peripheral side of the second cut. The direction of degeneration is always the direction of growth, so this experiment shows that the growth of the new fibres had not started from the periphery centralwards, but in the reverse direction. On looking up the literature of the subject, I found that Vulpian also did this experiment with the same result, and it can hardly be doubted that this formed one of the factors that later led him to abandon the autogenetic theory. An experiment on somewhat the same lines has been carried out recently by Lugaro : he has shown that regeneration of the cut nerves connected with the lower part of the spinal cord does not occur after that part of the spinal cord has been extirpated. This is a very striking piece of evidence, showing the dependence of the growth of fibres on the activity of the cells of the central nervous system with which they are originally connected. Ocular evidence is, however, perhaps more convincing than the description of experimental results, so I will next present to the readers of Science Progress drawings of some of the microscopic specimens prepared by the latest improved methods.

Cajal, by the help of his new silver method, comes to the conclusion that the new formation of nerve axons in the peripheral stump is exclusively due to growth from the central end.

He figures the long and often contorted course of these growing fibres in the swelling at the cut central end, and shows that they ultimately reach their goal—the peripheral segment—in

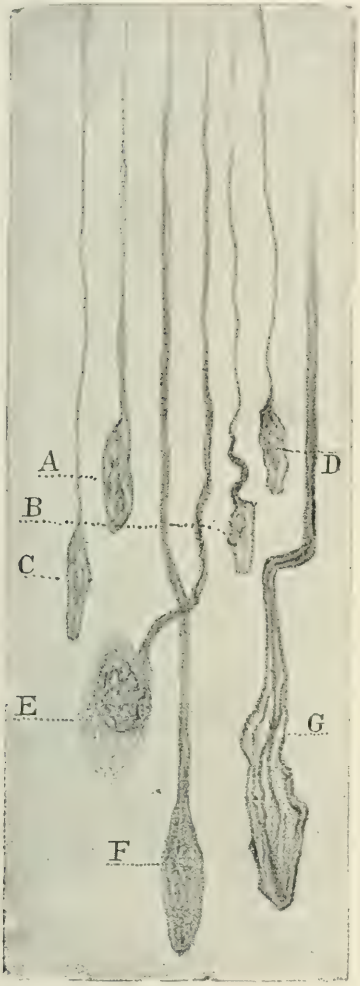


Fig. 1.—Olive-shaped swellings at the ends of nerve fibres growing distalwards from the central ends twenty-one days after the nerve had been divided. (Marinesco.)

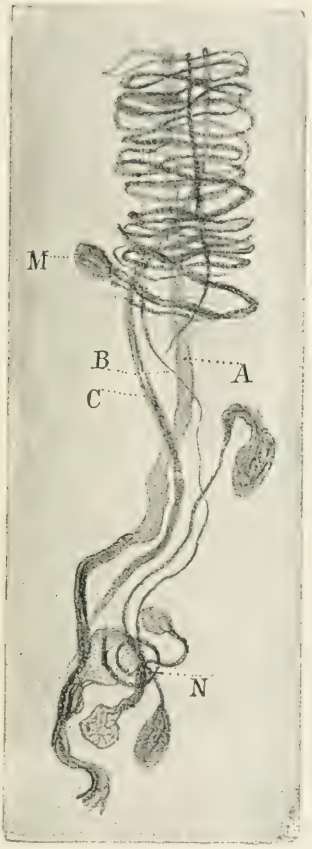


Fig. 2.—Spiral forms often seen in regenerating nerve fibres: the fibres are seen to be of varying thickness, and each is provided with a terminal swelling. (Marinesco.)

time and in spite of all hindrances. The greater the obstacles interposed the later does the union and consequent regeneration in the peripheral end occur. He also draws attention to the

olive-shaped swelling at the free end of each growing axis-cylinder. These are also figured by Marinesco, and I am indebted to the kindness of Prof. Marinesco for figures 1 and 2, which illustrate these swellings; in figure 2 the convoluted course of the new fibres, which is so frequently seen, is shown.

Marinesco calls attention also to the fact that these terminal swellings, although they may roughly be described as olive-shaped, vary a good deal in external form; this is shown in the drawings, and is quite intelligible now that we have Ross Harrison's description of the constant changes of form they exhibit in embryonic history. In regenerating fibres we have not direct evidence that similar amœboid movements take place, for the preparations were made not while the fibres possessed vitality but after fixation with a suitable hardening reagent. Still, the mere fact that they are fixed in various shapes is conclusive evidence that they do exhibit these changing outlines.

Marinesco has passed through the stage so often seen in those who have attacked the problem of nerve repair. In his earlier publications he thought his preparations afforded evidence of auto-regeneration, though he never went as far as Bethe has done in that direction. In his latest papers he has entirely recanted these views, and is now convinced that auto-regeneration is a myth. I reproduce two more of his very beautiful illustrations. The preparations were stained by Cajal's new method, and they require but little comment.

They show the new fibres penetrating the cicatricial tissue of the junction from the central end in a peripheral direction; they show the absence of any new axons developed auto-genetically in the peripheral end. Such preparations ought to carry conviction to those who have any lingering belief in auto-regeneration, that the Wallerian view is the only possible one to adopt.

It must not, however, be supposed that the peripheral end is entirely inactive; for while degeneration is progressing in the axons and their fatty sheath, an active multiplication of the cells of the primitive sheath or neurilemma is taking place. These neurilemmal cells probably play a nutritive action towards the more important structures within them, and Graham Kerr, in a recent study of nerve growth in the fish *Lepidosiren*,

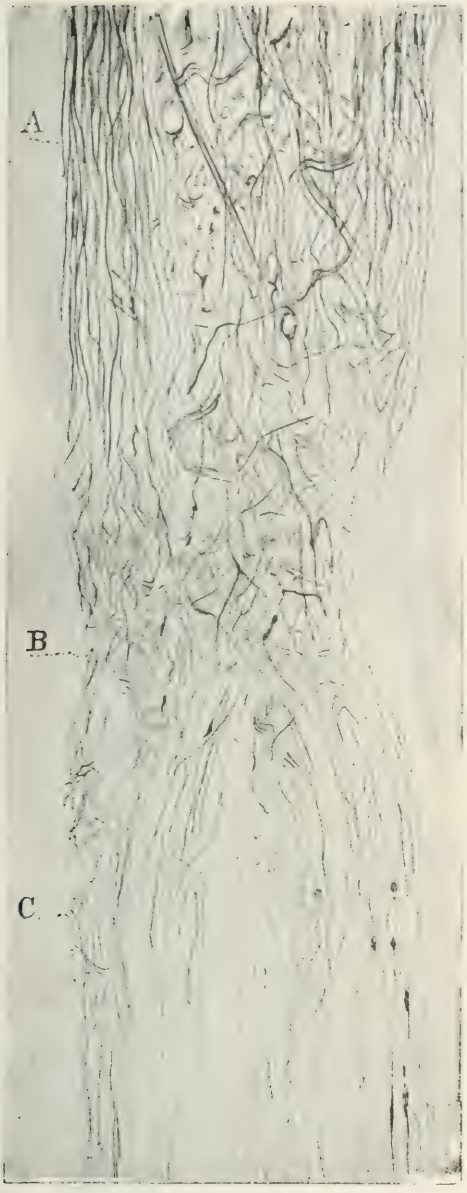


Fig. 3.—Longitudinal section of sciatic nerve of new-born dog eight days after the nerve had been divided. Union had already taken place. At the extremity of the central end (A) growing fibres are seen running in various directions towards the cicatrix (B); C is the peripheral end, into which a few fibres are already penetrating. (Marinesco.)

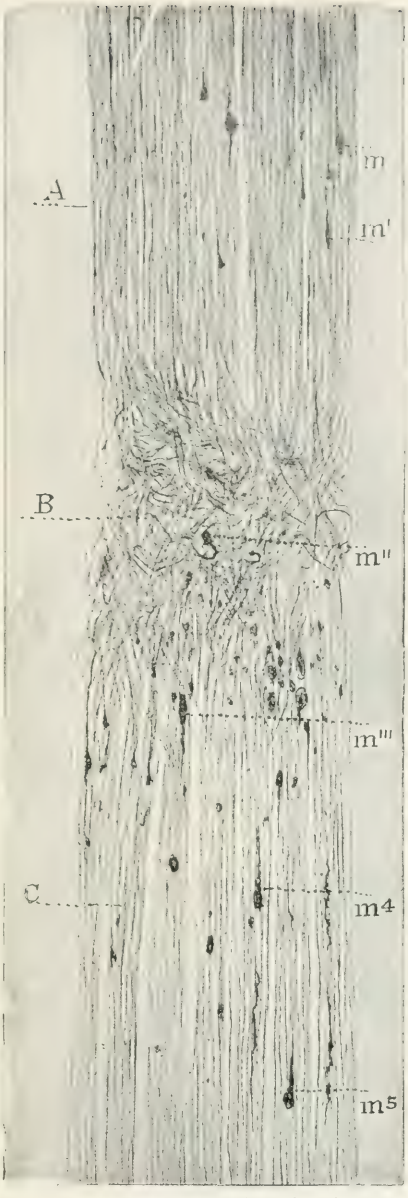


Fig. 4.—Longitudinal section of dog's nerve, twenty-one days after the nerve had been divided. A more advanced condition is seen than that shown in fig. 3. A is the central end; B the cicatrix of union, where the nerve fibres are seen in great numbers taking an irregular course. Into C, the peripheral end, many fibres have already successfully penetrated; m, m', m'', etc., are the terminal swellings of fibres growing from the central stump and directed towards the periphery, except in the case of m'', where the twisting of the new fibre in its efforts to grow had, at the time when the animal was killed, directed the termination backwards. (Marinesco.)

has supported in a very conclusive and entirely independent way the view that Mott and I advanced some years ago of the value of the neurilemma in maintaining the nutrition of the axis cylinder. There is but little doubt also that these cells act as phagocytes in the removal of the degenerated products of the other portions of the nerve fibre. But after this is accomplished they elongate and unite into long chains. It is this appearance that has led some observers into regarding them as true nerve fibres; they have jumped to the conclusion that the neurilemmal cells are also able to form a conducting core, and so have regarded auto-regeneration as a histological possibility. But all recent observations by the best methods, as I have already stated, have failed to discover either an axial core or a fatty sheath in these "embryonic fibres" as they have been termed. Howell and Huber put it very well nearly twenty years ago when they said the peripheral structures are able to prepare the scaffolding, but the axon, the essential conducting core of the fibre, has an exclusively central origin.

The change in the neurilemmal cells which occurs in the peripheral segment is even more vigorous at the central termination of the cut nerve; here its nutritive function (or apotrophic function, as Marinesco calls it) is effective, and provides for the nourishment of the actively lengthening axis cylinders. At the peripheral end, unless the axons reach it, it is ineffective in so far as any real new formation of nerve fibres is concerned. If, however, the axons reach the peripheral segment, the work of the neurilemmal cells has not been useless, for they provide the supporting and nutritive elements necessary for their continued and successful growth. Moreover, the neurilemmal activity appears to be essential. In the white fibres of the central nervous system the neurilemma is absent; in this situation not only is the removal of the products of degeneration a very slow process, but, as I have previously mentioned, regeneration does not occur.

Having now considered the manner in which nerve regeneration occurs as elucidated by experiments upon animals, we can next pass to a study of the problem in man. When the accident is a slight one, or when a surgeon during an operation necessarily divides a number of small cutaneous and similar nerves, the process of repair may be confidently left to nature. But when a large nerve trunk is divided, the case is a different

one—for a large nerve on division curls up at the end, forming a contorted leash of fibres; it also retracts, and a considerable amount of scar tissue is formed. This cicatricial tissue, by the time the nerve has begun to regenerate, has become very dense, and so an impediment to the successful growth of the nerve fibres is interposed. Still, in spite of such a barrier, spontaneous restoration frequently takes place. If the cut were made by a dirty piece of sharp machinery, inflammation usually comes in as a complication, and causes still further delay. In animals, where the cut is made with a clean knife, and antiseptic precautions are employed, this danger is reduced to a minimum, the amount of scar tissue is less, and union may be anticipated without suture. But even here suture hastens recovery, and in man thorough cleansing of the wound and the union of the divided nerve by appropriate ligatures should always be carried out. The growing nerve fibres will then not be left to themselves to find their way into the peripheral end, but will be guided more rapidly to their destination. In a number of cases where the carelessness of the patient has caused him to neglect obtaining this surgical assistance, it may be years before restoration of function takes place, but even under these circumstances the surgeon can help him. The old wound is opened up, the swollen lumps at the ends of the two segments of nerve are removed, and the two ends can then be united; the operation is spoken of as "secondary suture," and recovery is then comparatively rapid.

In some cases where the gap is a long one, some surgeons have been in the habit of transplanting a piece of nerve obtained in a living condition from a recently killed animal to bridge across the interval. This, however, is not to be recommended, since Marinesco has shown that if a transplanted nerve is obtained from another species of animal it is completely necrosed, just as the blood corpuscles of one animal are destroyed by the body fluids of another. The interposition of a foreign piece of nerve is thus in man rather a hindrance than a help.

Many surgeons have been steadfast upholders of the autogenetic theory because they have found in man that sensation may return very rapidly after freshening up and suturing together the ends of a nerve which had been divided a long time previously. They argue that during this time new nerve fibres

must have been produced in the peripheral segment, which would be ready to exercise their functions directly they are placed in continuity with the fibres of the central end.

Examination of these cases shows, however, that sometimes the conclusion has been a hasty one, depending largely on the evidence of the patients themselves, in whom the wish is father to the thought. There was a case of this kind recently in King's College Hospital. Very shortly after the operation the patient stated he was able to feel, but these sensations rapidly subsided, and sensation did not really return until months later. The preliminary sensations were doubtless subjective; the irritation of the nerve in the scar lasted some hours, and the sensations so produced were referred by the patient's mind to the original terminals of the fibres.

In other cases the genuineness of the early recovery is doubtful, because insufficient care is taken in testing the patient's sensations. In spite of sensitiveness to such tests as needle pricks, there is usually absolute anæsthesia to the far more delicate test of stroking the hairs over the affected region until quite late dates. Head and Sherren have recorded a large number of surgical cases, but evidence of early recovery is usually lacking; there is some difficulty in localising the stimulus so that it should not affect the hyperæsthetic marginal zone of the anæsthetic region, and if this danger is recognised it can be avoided. Another pitfall is this: the deep sensibility of the subjacent parts, such as would be excited by needle pricks, or by pressure, is entirely independent of cutaneous sensation. The fibres subserving this form of sensation run mainly with the motor nerves, and are not destroyed by division of all the nerves to the skin. Neglect to recognise this fact will explain some of the results of those who have published cases of early recovery.

The difficulty of clinical observation on others induced Dr. Head to combine with his experiments on animals and his observations on patients, an experiment on one of the sensory nerves of his own arm. This gave an opportunity for the study of the phenomena by a trained observer upon himself. He certainly experienced no early return of function, and the date at which sensation did come back coincides closely with the dates obtained in animals by physiologists for the reappearance of new and fully formed nerve fibres.

In the course of this work he made the interesting dis-

covery that the first kind of sensation to return is of a vague nature, which he terms *protopathic*; he associates this with the activity of the fine medullated fibres which replace the degenerated ones at an early date; return of protopathic sensibility begins about the eightieth day. The more elaborate sensations and power to localise them accurately return much later, and this *epicritic sensibility*, as Head terms it, is usually not perfect until months, or even years, have elapsed after the regeneration started.

By this time, as was shown by experiments on animals, the fine nerve fibres which subserve protopathic sensation are largely admixed with a later growth of larger nerve fibres, and he believes that epicritic sensation is subserved by these. He also postulates that the three kinds of sensation (deep, protopathic, and epicritic) are related to different kinds of end-organs in the peripheral structures.

Granting that earlier observers had been led into errors by not being acquainted with Head's discoveries, and that many cases of supposed early recovery can be explained in the manner already indicated, there remains a small residuum of cases in which such explanations do not hold, and in which early recovery appears to have been an undoubted fact. Of these, I would particularly refer to some cases recorded by Dr. Kennedy of Glasgow, in which the return of sensation of some sort, especially to painful stimuli, was a matter of only a few days. In these cases due care appears to have been taken to exclude any bias of the patient's own mind. He admits that such cases are rare; from his own point of view auto-regeneration would therefore appear to be an unusual phenomenon. Granting that the early recovery was genuine, it seems to me there is a much more probable explanation than that of auto-regeneration. They are doubtless instances where extremely rapid growth of new axis cylinders from the central into the peripheral segment of the nerve took place. Dr. Kennedy's cases appear to be happy examples of the date of the secondary suture being such that the usual impediments to repair were minimal. The preparatory work of the neurilemmal cells in the distal portion of the nerve had evidently reached that stage when they were most fitted to receive the new axis cylinders, and by their metabolic activity favour the onward growth of the fibrils at a rapid rate. The examination of

Dr. Kennedy's own records supports the same view. In three of his four cases sensation returned first in the parts nearest the suture, and later in more distant parts, such as the finger tips. This surely proves that regeneration was occurring in a peripheral direction. The record of the fourth case gives no details of the order of recovery. The only difficulty remaining is the unusual rapidity of the reparative process. I say "unusual" because most of our ideas of the rate at which axis cylinders grow are derived from cases in which gross impediments to growth are present. I have, however, been struck in some of our own observations with the rapidity with which the central unites with the peripheral end, even when an inch or more of nerve has been excised. In some cases there was a well-defined strand of union after a few days. This is especially the case when young and vigorous animals are employed. In two of Dr. Kennedy's cases, the patients were in the prime of life (aged twenty-nine and thirty-seven respectively); the other two were children—aged six and sixteen respectively.

The introduction of the new methods of Cajal has now furnished us with a method of tracking the growing axis cylinders with a certainty which was previously lacking; and Marinesco, availing himself of these methods, has shown that the lengthening of the regenerating fibres is demonstrable twenty-four hours after a nerve has been cut.

This concludes what I have to say on an important problem of what we may call applied physiology. In the present state of our knowledge it is impossible to avoid controversial questions. The proverbial disagreement between doctors is specially true of physiologists, and really this is only as it ought to be. Whilst knowledge is in the making there must be divergencies of opinion, and it is only by a full and free discussion of these, and by the renewed research of which criticism is the stimulus, that any certainty in the way of truth can be arrived at.

MENDELISM

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Perhaps the most interesting, certainly the most sensational, chapter in the history of biology, when it comes to be written, will be that which deals with the discovery of Mendel's papers and with the fruit which that discovery has borne. We are already in possession of all the material for the former topic; and the first instalment of the fruit which has already been harvested augurs well for the quality of what is to come.

In 1865, a man absolutely unknown to the biological world, Gregor Johann Mendel, read before a meeting of the Natural History Society of Brünn a paper on his experiments with Peas, entitled "Experiments with Plant Hybrids." To-day no name is better known to biologists than that of Mendel. And yet, when he died in 1884, he was as obscure as when he read his famous paper. Indeed, it was not until sixteen years after his death that Mendel suddenly burst from obscurity into fame.

In 1900 Mendel's experiments with Peas were repeated, and his results confirmed, independently by De Vries in Amsterdam, Correns in Leipzig, and Tschermak in Vienna. This spark—the discovery of Mendel's paper—has been responsible for the generation of an enormous amount of energy, which has manifested itself ever since in a continually smouldering train of investigation; and on one or two occasions in explosions of controversial violence.

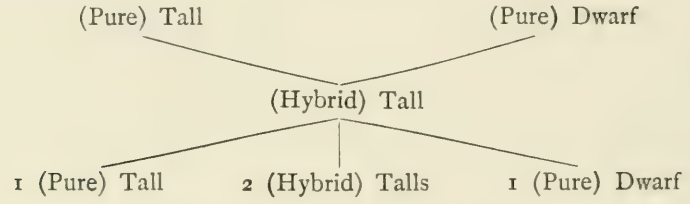
We must now make it our business to become familiar with the facts which Mendel discovered, and with the interpretation which he put upon them. Mendel found that varieties of the culinary pea, *Pisum sativum*, could be classified according to a number of constant, true-breeding characteristics, such as the form of the plant, whether normal (*i.e.* tall) or dwarf; or such as the colour of the cotyledons of the seeds, whether yellow or green; and so forth.

Mendel crossed a Tall with a Dwarf. The resulting hybrid

was invariably a Tall. Tallness, therefore, he called the *dominant* character and dwarfness the *recessive*.

These hybrids were allowed to self-fertilise (the normal process in *Pisum*), and seeds gathered from them gave, when grown, 1064 plants, of which 787 were tall and 277 dwarf; that is to say, the hybrid Talls produced Talls and Dwarfs in the proportion of 2·84 to 1, or roughly 3 to 1.

The seeds of these plants were sown, and it was found that all the Dwarfs bred true, whilst of every three Talls, one produced only Talls, the other two giving rise to Talls and Dwarfs in the proportion 3 to 1. The foregoing account may be summarised in the following diagram:



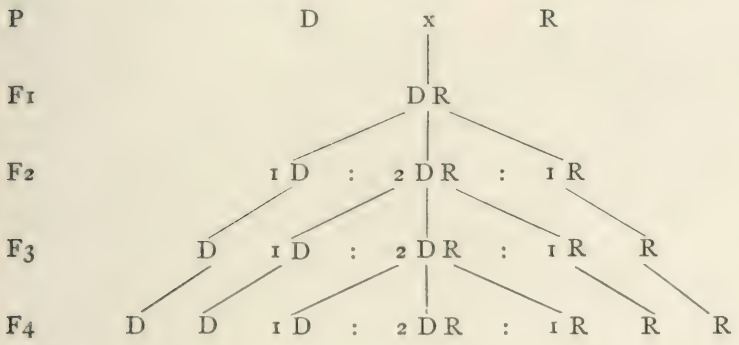
Let us now look at the three kinds of Peas which compose the generation in which Pure Talls, Hybrid Talls, and Pure Dwarfs appear in the proportion 1 : 2 : 1. We already know that all the Dwarfs breed true, that a third of the Talls breed true, whilst the rest of the Talls produce Talls and Dwarfs in the proportion 3 to 1. This proportion—3 to 1—is made up in the same way as the same proportion in the preceding generation—namely, 1 Pure Tall; 2 Hybrid Talls; and 1 Pure Dwarf. All these Dwarfs breed true; all the Pure Talls breed true; and the Hybrid Talls produce Talls and Dwarfs in the proportion 3 to 1. And so on.

There are two sets of symbols which are very convenient in describing these phenomena.

The first set describes the different kinds of individuals; the second the generations. D denotes the dominant form (in the case we have been discussing, the Tall), R the recessive (the Dwarf), and D R the hybrid.

P denotes the parental generation; F1, the result of crossing the two parent forms—in our case, the Hybrid Tall; F2 denotes the next generation, *i.e.* that in which the dwarfs reappear in the proportion 25 per cent.

We may now summarise the Mendelian phenomenon in the following diagram:



What is the cause of this beautifully regular and definite phenomenon? Mendel answered the question in this ingenious way. He believed that the germ-cell of the hybrid pea did not represent a mixture of tallness and dwarfness, but that a germ-cell represented *either* a tall *or* a dwarf, and, moreover, that half of the germ-cells represented tall and half of them dwarfs. Let us see what is the result of the union of—say—a hundred male germ-cells, or pollen-cells, with a hundred female germ-cells, or egg-cells. Before we proceed, let us make certain of two points which are essential to the argument. Mendel believed that a pure-breeding tall pea contained only tall-representing germ-cells—in fact, that the reason that it bred true was that it did contain them; and that the dwarf contained only dwarf-representing germ-cells. Mendel had also found that the result of a union between a "tall" germ-cell and a "dwarf" one was a hybrid which bore the tall character. Now let us return to our argument.

Fifty of the hundred pollen-cells would be "tall," and fifty "dwarf." Let us consider the "talls" first. As half of the eggs-cells are "tall" and half "dwarf," the "tall" pollen-cell has an equal chance of meeting a "tall" and a "dwarf" egg-cell. So that twenty-five of the "tall" pollen-cells would meet "tall" egg-cells, and twenty-five would meet "dwarf" ones; and the resulting unions would be (writing T for Tall, and D for Dwarf):

25 T T
25 T D

Similarly twenty-five of the fifty "dwarf" egg-cells would meet "talls" and twenty-five "dwarfs," or briefly:

25 D T
25 D D

The total result therefore is:

25 T T
25 T D
25 D T
25 D D

Now we already know that the result of the union of a "tall" with a "dwarf" germ-cell is a hybrid which is tall, and Mendel found that it made no difference whether the tall character was carried by the male parent and the dwarf by the female, or *vice versa*. So that the above result can be written:

25 T T (Pure Talls)
50 T D (or D T) (Hybrid Talls)
25 D D (Pure Dwarfs)

The result of the random union of two sets of germ-cells, each set consisting of 50 per cent. of germ-cells bearing the Recessive and of 50 per cent. bearing the Dominant character, can be admirably illustrated by drawing one counter at random from a hat containing fifty Red and fifty White counters, and another from another hat with similar contents, and by placing the three kinds of pairs in three separate rows on the table, according as whether they are R W, R R, or W W. The larger the number of trials you make the closer will the approximation to the proportion of 25 R R, 50 R W, 25 W W be. Counters are worse than marbles for illustrating this, by reason of the difficulty of thoroughly shuffling them. But they are better than them, because the phenomenon of dominance and the proportion 3 : 1 can be illustrated with them, the former by merely making it a rule to put the red counter on the top of the white when a red and a white are drawn together, the latter by superposing the upper counter in every pair so carefully that the lower one is invisible. If this is done effectively there will be seen, at any rate from a slight distance, to be 75 per cent. red counters and 25 per cent. white. But if you jog the table the difference between the previously apparently identical reds will be revealed. It is also very

easy to illustrate subsequent results with the counters. The result of mating the hybrids in F2 can be illustrated in exactly the same way as that of mating the F1 hybrids. And it is readily seen that it is no more possible to get anything but Dwarfs from the Dwarfs in F2 than it is possible to get anything but pairs of white counters by drawing from two hats each containing nothing but white ones, and that it is no more possible to get anything but Talls from those Talls, *i.e.* the pure ones, which resulted from the union of like gametes than to get anything but pairs of reds from drawings from two hats containing only reds.

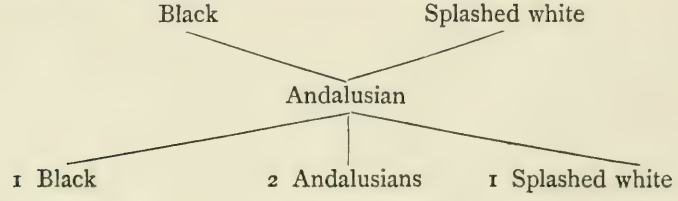
The animal or plant resulting from the union of two germ-cells or gametes—and all animals and plants which reproduce sexually result from such a union—is called a *zygote*; that resulting from the union of like gametes or germ-cells is called a *homozygote*; that from the union of unlike gametes a *heterozygote*.

The heterozygote in the case which we have been considering as an example was only distinguishable from the dominant homozygote by the fact that when allowed to self-fertilise it produced Dwarfs as well as Talls. But sometimes the result of crossing two pure strains is different from either of them—that is to say, the heterozygote has a character peculiar to itself. This makes it possible to distinguish between the heterozygotes and the dominant homozygotes in F2 otherwise than by breeding from them.

The phenomenon may be illustrated by the famous case of the Andalusian fowl. If you buy a pair of these birds from the very best source you will find that they will not breed true, but that they will produce besides the Andalusians blacks and splashed whites, and that on the average these three forms will be produced in the proportion 1 black, 2 Andalusians, 1 splashed white. If you were not acquainted with Mendelian phenomena, but were guided by the principles of heredity which were universally held in pre-Mendelian days (and are still by those who are not Mendelians), you would sell the blacks and whites for eating and breed from the Andalusians, in the belief that, as they at least, to your own personal knowledge, had an Andalusian father and mother, they would be more likely to breed true than a pair of Andalusians of unknown and, as you would expect from the result

of mating them, of mixed parentage. Your anticipation would not be fulfilled. Your Andalusians would again produce blacks, Andalusians, and splashed whites in the proportion 1 : 2 : 1. But if you were acquainted with Mendelian phenomena you would immediately suspect that the Andalusian was a heterozygote form, and that the two homozygote forms which by their union gave rise to it were the blacks and the splashed whites. This is indeed the case. Mate the black with the splashed white, and you get nothing but Andalusians. The obvious plan, therefore, instead of selling the blacks and whites to the poulterer and keeping the Andalusians to breed from, and then only getting 50 per cent. of what you want, is to sell the Andalusians to fanciers, keep the blacks and whites, and get 100 per cent. of Andalusians by crossing them, making occasional matings of Andalusians to keep up the supply of blacks and whites.

This case is summarised in the following diagram :



Let us now return to the Peas. We have referred in detail to only one pair of characters, namely, Tall and Dwarf. Mendel dealt with six other pairs of characters, namely :

1. The shape of the ripe seeds, which was either round or wrinkled.
2. The colour of the ripe cotyledons in the seed, which were either yellow or green.
3. The colour of the seed-coat, which was either white or grey (with or without purple spots).
4. The texture of the ripe pods, which was either hard and inflated or soft and constricted between the seeds.
5. The colour of the unripe pods, which was either green or yellow.
6. The arrangement of the flowers on the stem. The flowers either came off normally at the nodes along the stem, or they were situate in a bunch at the top of the stem ; this latter is the so-called Mummy habit.

The two members of each of these pairs of character stand to one another in the relation of dominant and recessive. In the above list the dominant member of each pair is written first. The two characters which form such a pair are called *allelomorphs*; and green is said to be allelomorphic to—*i.e.* forms the other member of a pair with—yellow. Similarly yellow is allelomorphic to green.

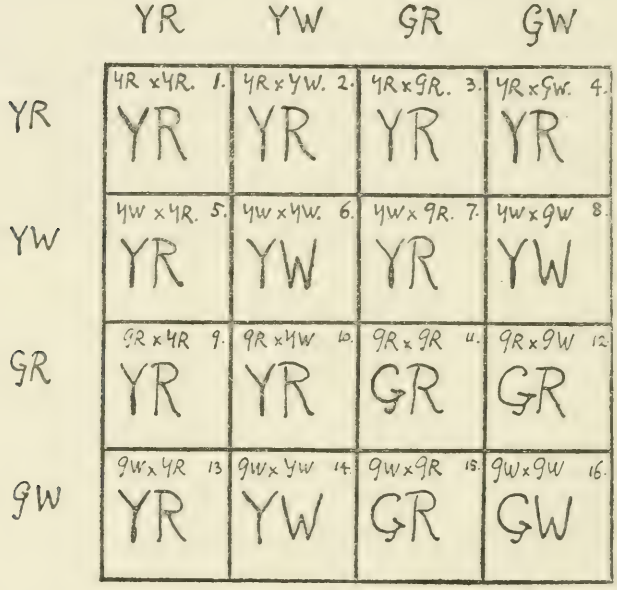
We have so far, in considering the results of crossing an animal or a plant, paid attention only to a single pair of allelomorphs, tall and dwarf, black and splashed white. We must now ask ourselves, What is the relation between character belonging to different allelomorphic pairs? And the answer, broadly speaking, is that in the case of some characters there is no relation, whilst in the case of others there is; or, to amplify the answer, in the case of some characters the members of distinct allelomorphic pairs exercise a very definite effect on one another, whilst in the case of others they are entirely independent of one another. We will deal with the latter case first, because it is simpler.

What the result of mating a green round with a yellow wrinkled pea will be, can be answered by reference to the table of characteristics on p. 434, in which (it will be remembered) the dominant member of a pair is written first. The result will be a yellow round one. What the result of allowing these to self-fertilise will be can be predicted from the result of making drawings of two counters from each of two hats, which contain equal numbers of green and yellow, red and white counters.

The number and nature of the possible combinations is given by the number and contents of the squares in the Table on p. 436.

Y and G stand both for yellow and green counters and "yellow" and "green" germ-cells; R and W stand both for red and white counters and "round" and "wrinkled" germ-cells. The four pairs of letters along the top of the table represent the four kinds of male gametes; the pairs along the side the four kinds of female ones; the squares represent zygotes. Let us consider the results of their union, beginning at the top left-hand square. (In the following account Y is to be read as yellow, G as green, R as round, and W as wrinkled.) Here we have to do with union between two Y R gametes. The result is a pure Y R zygote. In the square to the right of

it, it is a case of a union between a Y R egg-cell and a Y W pollen-cell; the result is a Y R zygote, which is a homozygote regards colour and a heterozygote as regards shape. In the next square to the right a Y R gamete meets a G R gamete; the result is again a Y R zygote, which differs from the last in that it is homozygote as regards shape and heterozygote as regards colour. In the last square on the top line a Y R gamete meets a G W one; the result is a Y R which differs from any of the preceding ones by being heterozygote both as regards



colour and shape. And so on throughout the table. The nature of the sixteen possible kinds of unions of gametes is written in small letters in the sixteen squares of the table. The characters of the resulting zygotes are written in large letters. When this explanation is being illustrated with counters, the nature of the zygote in each case is given by making it a rule always to put a yellow on the top of a green counter and a red on the top of a white.

The result therefore of allowing our Y R hybrid (produced by Y W and G R) to self-fertilise should be:

9 Yellow round (squares 1, 2, 3, 4, 5, 7, 9, 10, and 13).

3 Yellow wrinkled (squares 6, 8, and 14).

3 Green round (squares 11, 12, and 15).
5 Green wrinkled (square 16).

Or, briefly, 9 Y R, 3 Y W, 3 G R, 1 G W in every sixteen peas.

Of the 9 Y R, only 1 (square 1) is homozygous in both respects, and should produce only Y Rs. Two Y Rs are homozygous in colour only, but heterozygous in shape (viz. squares 2 and 5); they should produce only yellow, but both "Rounds" and "Wrinkleds." Two Y Rs are homozygous in shape, but heterozygous in colour (viz. squares 3 and 9); they should produce only rounds, but both "Yellows" and "Greens." The remaining 4 (squares 4, 7, 10, and 13) are heterozygous both in colour and in shape, and should produce all 4 kinds—Y R, Y W, G R, and G W.

On the 3 Y W, 1 (square 6) is homozygous in both shape and colour, whilst the remaining 2 (squares 8 and 14) are homozygous in shape only. Similarly with the 3 G Rs, that in square 11 is homozygous in both respects, the other 2 (squares 12 and 15) being homozygous in colour only. It will, of course, be noted that the character in respect of which all the 3 Y Ws and all the 3 G Rs are homozygous is a recessive one—namely, wrinkledness in Y W and greenness in G R.

There is only 1 G W zygote which, since both its characters are recessive, is therefore homozygous in both respects.

A very close approximation to the result 9 Y R, 3 Y W, 3 G R, 1 G W has been obtained in experiments by Mendel, Bateson, Hurst, and myself. And the various types of Y R, Y W, and G R have also been recognised.

Another point of view from which the proportion 9 : 3 : 3 : 1 may be regarded is from that of the zygotes solely. If we have a generation which displays the two characters of one pair Y and G in the proportion 3 Y to 1 G in every 4, and the two characters of another pair R and W in the proportion 3 R to 1 W in every 4, and if we further suppose that the two pairs are independent of one another, we should expect that in every 16 (= 4 × 4), 9 (= 3 × 3) would be Y and R, 3 (= 3 × 1) Y and W, 3 (= 3 × 1) G and R, and 1 (= 1 × 1) G and W. And we find that this is so, which shows that the two pairs are independent of one another.

Having become familiar with the signification of the 9 : 3 : 3 : 1 proportion, let us proceed to the discussion of other results which can only be interpreted in the light of it. We need not

leave the Pea for illustration. If we ask a corn-dealer for a pennyworth of Maple or Partridge Peas, he will give us a kind of pea the seed-coat of which at first glance appears a uniform brown, but on closer inspection is seen to have a ground-colour of a pale brown, on which is a very beautiful mottling, consisting of anastomosing tracts of a rich brown colour. This type of coloration is called mapling. Another type of coloration is that which is often seen on (though it is not necessarily associated with) the kind of pea which is much grown and eaten on the Continent, and is known as the Sugar Pea. In this type of coloration there are minute spots (discernible by the naked eye) of slightly varying sizes and of a rich purple colour on a greenish grey background. The former type will be referred to as "maple" and the latter as "purple spot."

When a pea with a maple seed-coat is crossed with one with a purple spotted coat, the result is a pea on whose seed-coat both mapling and purple spots exist. This suggests that maple and purple-spot are not allelomorphic to one another, but belong to separate pairs which are supposed to be maple (M) and not-maple (nM), and purple spot (P) and not-purple spot (nP). This sounds very much like a logical exercise, a matter of words and not of things. But the reality of it is shown by breeding from the hybrids; for by doing this we actually get peas, which we can touch and see, which exhibit neither mapling nor purple spotting. We get, in fact, the following four types of peas in the proportions given by the numbers which precede them:

9 M P, 3 M, 3 P, 1 grey.

If we write this in a form analogous to—

9 Y R, 3 Y W, 3 G R, G W—

we should have:

9 M P, 3 M nP, 3 P nM, 1 nP nM.

The nP nM is grey because all these colours are on a background of pale grey. This proportion shows that we are dealing with two independent pairs of characters, viz.:

1. Maple and not-maple.
2. Purple spot and not-purple spot.

Now we come to those cases in which one of the characters

of a pair of allelomorphs is not independent of a character in another pair.

We will cross a pea with a pale grey seed-coat, such as that which formed the background for the purple and the maple (many field peas exhibit no more than this grey colour), with a pea with no colour at all in its seed-coat, as is the case in a great number of the peas grown for the table. The result is a grey-seeded pea *on which there are purple spots*, which existed in neither parent. Heretofore this would have been labelled as an example of reversion and the matter would have been regarded as settled, or at any rate done with. But to call a thing reversion does not make one any wiser about it. Mendelism has provided a reasonable explanation of reversion in this case. Let us see how. The result of breeding from these grey-coated purple-spotted hybrids provided the clue. There were produced 9 grey with purple spots, 3 grey, and 4 white.

What does this mean? The 9 : 3 : 3 : 1 proportion suggests an explanation. It is supposed that here, as in the case of the cross between the maple and the purple spot, we are dealing with two pairs of allelomorphs, namely, (1) grey (G), and not-grey (nG), and (2) purple spot (P) and not-purple spot (nP). The grey pea exhibits the dominant grey character (G) of that pair. The white pea possesses—and here we come to the case of the interdependence of characters in separate pairs—the dominant purple spotting (P) of that pair; *but the purple spotting is not exhibited because one of its properties is that it cannot be manifested unless associated with the grey coat.* Bearing this theory in mind, let us write the 9 : 3 : 3 : 1 scheme for the two pair of allelomorphs G and nG, P and nP. It will be:

9 G P, 3 G nP, 3 P nG, 1 nP nG.

But we see that in the 3 P nG the purple spot is there, but the grey coat is not. Therefore *ex hypothesi* it will not be manifested and these three seeds will appear white, which gives the proportion 9 grey with purple spots, 3 grey and 4 white.

The possession by the hybrid of a character which neither of its parents possesses is accounted for by supposing that that character (in this case purple spotting) depends for its manifestation on two factors (in this case P and G), one of which exists in one parent and the other in the other. This Mendelian hypothesis therefore enables us to account for the reversion and for the

otherwise meaningless proportion 9 : 3 : 4; or perhaps we should not say more than that the facts are consistent with the truth of this theory.

We will now pass from the culinary to the sweet pea, and describe Bateson's beautiful experiment. One of the countless varieties of the sweet pea grown to-day, Emily Henderson, a pure white, is remarkable for the fact that, whereas the pollen grains of most other varieties are cylindrical with almost parallel sides, some plants of it have this ordinary "long" pollen whilst others have *irregularly round pollen.* The irregularity is never so much pronounced that the round pollen can for a moment be mistaken for the long. It may help those familiar with peas and beans to say that the contour of the round pollen is never more irregular than that of the most irregular wrinkled peas, whilst that of the long pollen may be compared to a stout example of the dwarf bean Non Plus Ultra. Bateson crossed the long-pollened form of Emily Henderson with the short. *The result was a sweet pea of a deep purple colour.* The purple of the standard, the single outer erect petal, was of a redder shade than that of the wings, the two petals just below the standard. This sweet pea is known in the fancy as Purple Invincible, but is seen more often now in cottage gardens than in up-to-date collections. Its type of coloration is that of the original sweet pea as first brought into cultivation. The wild *Lathyrus odoratus*—of the Purple Invincible type—is common in Sicily. Here then is an extraordinary case of reversion. Two pure white sweet peas differing merely in the shape of their pollen are crossed, and a hybrid with the coloration of the ancestor of all cultivated sweet peas is the result. When these hybrids are allowed to self-fertilise, there results in F_2 a variety of forms already well known to the fancy. These are: Purple Invincible again, Picotee, Painted Lady, and Tinged White and pure whites. Passing over the proportions in which the various coloured forms appear, there are found to be 9 coloured to 7 white.

Now, the 9 : 3 : 4 proportion in the culinary pea suggested the existence of two pairs of characters, one of which—the purple spot—could not be manifested in the absence of the other, the grey coat; though the grey coat could be manifested in the absence of the purple spot. The 9 : 7 is accounted for by supposing that colour in the sweet pea depends for its existence upon the simultaneous presence of two factors of distinct

allelomorphic pairs, *neither of which* can be manifested in the absence of the other. These factors have been called C and R, their absences c and r respectively. If we write the familiar 9 : 3 : 3 : 1 scheme again, we get

9 C R, 3 cR, 3 Cr, 1 cr.

Only in the 9 C R are both C and R present; in the 3 cR and the 3 Cr only one of them is present, whilst in the 1 cr neither of them is. So that only the 9 are coloured and the remainder are all white.

The actual proportion of purples and reds and whites in F2 in this instance is 27 purple, 9 red, 28 white (= 36 coloured to 28 white, *i.e.* 9 coloured to 7 white). The existence and proportions of these reds are accounted for by calling in a new pair of allelomorphs, B and b. As already stated, colour is due to the meeting of C with R; if B is absent the colour is red, if present it is purple. But we will not deal further with this now.

Having now seen how these complicated cases of segregation, together with two instances of reversion, have been elucidated by Mendelian analysis, let us turn to other groups of the animal and vegetable kingdoms, and see what other characters have been dealt with in this way.

Except for the pair of allelomorphs with which we illustrated the simple Mendelian phenomenon, and for the very superficial reference to roundness and wrinkledness in peas, all the characters we have dealt with so far have been colour characters. But Mendelian inheritance is by no means confined to colour characters, as we shall shortly see. Perhaps one of the most striking instances of this is that studied by von Guaita, Weldon, and myself, the waltzing habit in mice. The waltzing habit characterises a race of mice known as Japanese waltzing mice, which have pink eyes and small patches of fawn on the shoulders and rump. A waltzing mouse does not always waltz, but can always be distinguished immediately from a normal mouse. The waltzing itself consists in merely spinning round and round very swiftly in one spot on all-fours. It is not in the least like waltzing. It is simply running round in a circle sometimes at a very great pace for several minutes at a time, so that all that you can see is a sort of nebulous ring of mouse. They do not always go round in the same direction,but sometimes

in a right-handed direction, sometimes in a left. But when it is not waltzing, a waltzing mouse is immediately recognisable by the appearance of complete lack of control over the movements of its head, which is every now and again thrown back into an apparently very uncomfortable and useless attitude. A waltzing mouse also has the habit of running or rather shuffling backwards, waving his head vigorously from side to side the while. This mouse is said to have come originally from Japan. They certainly exist there now, but I have not been able to obtain any alive from that country. The depth of the ignorance of the general public concerning the simplest facts of heredity is shown by the fact that the following story of the origin of the waltzing mouse has been printed in a daily paper and, presumably, believed. "A mouse fancier once caught the head of one of his pets in the door of his cage. The mouse nearly died, but on recovery exhibited the waltzing movements now so familiar. Wishing to perpetuate the curiosity, the owner of the mouse carefully preserved and bred from the anomaly, and so originated the breed of waltzing mice!" What actually happened was probably that the feature arose as a sport which was carefully preserved and bred from. Waltzing is, in fact, a very good example of the kind of character that can arise in a tame breed, and be preserved because it tickles the fancy of man. Nature does not tolerate curiosities of this kind for a moment, and promptly eliminates them. The abnormality is due, not, as was originally believed, to the absence or atrophy of one of the semicircular canals, which have been demonstrated to be all there, but to a deficiency in their nerve supply. The waltzing is a constant character of the race; the children of waltzing mice mated together being all waltzers.

When such a waltzing mouse is mated with a normal mouse —with an ordinary white mouse, for example—the hybrid obtained is always *normal*, like the white mouse. Waltzing, therefore, is recessive, in the Mendelian sense, to normality of progression. In F2 we should expect the waltzing to reappear in the proportion of 25 per cent. of the whole fraternity. Reappear it certainly does, but not quite in the proportion expected. The number of waltzers, indeed, falls below 20 per cent., in the many hundreds of F2 mice which I have raised. I do not think that this deficiency is due to any complication of the process of segregation in the germ-cells,

but believe it is merely due to the greater mortality which almost certainly takes place among waltzing individuals than among normal ones between the time when they are born and the age—from ten days to two weeks—when the characters of the mice are first registered. Waltzers are much more delicate than normal mice; and it is likely that this delicacy is more fatal in youth (perhaps even before birth) than in middle age.

The mice afford an excellent example of reversion. The Japanese waltzing mouse is almost an albino, and would be were it not for the patches of fawn-coloured fur on the shoulders and haunches. It has pink eyes. It was crossed with an albino mouse. The result, where the albino used was a pure-bred one, was a mouse hardly distinguishable at first sight from the common house mouse. A closer inspection reveals the lighter colour of the belly. Their eyes are jet black. And this is the most remarkable feature of the cross —the production of a black-eyed form from two parent forms both of which had pink eyes. These hybrids are not merely wild in their coloration, but are always noticeably healthier, stronger, and wilder than their albino parent, and infinitely more so than their waltzing parent. As already stated, they never waltz. When they are mated together they produce a generation which is composed of the three colour categories we have just mentioned in the proportions indicated by the percentages prefixed to them—viz. 25 per cent. pink eye and coloured coat (that of the Japanese waltzer), 50 per cent. dark eye and coloured coat (that of the hybrid), and 25 per cent. pink eye and colourless coat (*i.e.* complete absence of pigment or albinism.) The distribution of waltzing over these various colour categories is interesting. It is distributed, as it were, at random—that is to say, the waltzing character is not necessarily associated with that colour category with which it was associated in the pure waltzer, but may be associated with any of the three colour categories. So that we get in F_2, besides waltzers coloured like their pure grandparents, waltzers with the black eyes and the grey coat of the house mouse, and albino waltzers. It may be noted in passing that these albino waltzers correspond to the green wrinkled peas we have already discussed—that is to say, they represent the association in F_2 of the recessive character of one parent with the recessive character of the other parent in one individual

which, since both characters it bears are recessive, may be counted on to breed true. It does.

Another illustration of a character other than colour is that which has been studied by Mr. R. H. Biffen. One of the most disastrous pestilences which affect the wheat is the fungoid pest known as Yellow Rust (*Puccinia glumarum*). Some varieties of wheat are more susceptible to it than others; for example, the variety Michigan Bronze is hardly ever free from it. Another variety grown by Mr. Biffen may be regarded as practically immune, for, although it was grown for four years in close proximity to the most rust-stricken varieties, it was not affected by the disease. This immune variety was crossed with Michigan Bronze. The hybrids were all so badly affected by the disease that one could not find an area an eighth of an inch square unattacked by the rust. No difference in respect of their susceptibility to the disease could be discerned between the hybrid and Michigan Bronze. But the hybrid did differ from it by the fact that whereas the susceptible parent hardly set a grain, the hybrid gave a moderate crop.

"Every available grain was sown in plots alongside the parent varieties, part on land which had been exhausted on a previous crop of wheat, and part on land which had carried a crop of clover the previous season and consequently was in high condition. The difference in cultivation, however, made no difference in the results of the experiment. The rust was late in appearing that season, but again every plant of the susceptible parent was stricken, and all of the immune type escaped entirely. The hybrid plots were badly attacked, and when the epidemic seemed to be well advanced, the plants were sorted into two groups—namely, those which were attacked, and those showing no signs of the disease even on the withering basal leaves. On some plots the diseased plants were cut out, leaving those free from infection for a subsequent examination. A few individuals, which had escaped the attack at the early stage, became infected later, and these were then added to the total of susceptible plants. The statistics showed that 1,609 diseased plants were present and 523 immune, or a ratio of 3·07 : 1. There cannot, I believe, be any question that these latter were really immune, for they were surrounded by plants covered in rust, whose leaves were continually

rubbing against them. Further, if a variety is susceptible, no plant of it under field conditions ever appears to escape."[1]

Mr. Biffen concludes with the following remark: "Of the problems this opens up to the pathologist, nothing need be said here: for the present we are concerned with plant improvement, and it need only be noted that, according to the report of the International Phytological Bureau in 1890, the attacks of rust cost Germany some £20,000,000 sterling. Such figures give one some idea of the stakes the plant-breeder can now play for, and, thanks to the work of Mendel, with the reasonable certainty of winning."

But it is not merely by the discovery of facts of this kind that Mendelism is of service to the breeder of animals and plants.

Mendel and his followers have already put into the hands of breeders general principles of the greatest value. For example, those whose business is to breed cattle or carnations, or whose hobby is to breed fancy mice or sweet peas, and strive to improve their strains by crossing, are likely to be disappointed if they expect anything new in their first crosses. What could be more discouraging than to obtain a house mouse by crossing the delicate little waltzing mouse with the albino? And yet I have raised an entirely new colour—lilac, as it is called—by mating these hybrids together. Similarly in sweet peas the production of the original wild type of that flower by crossing two of the most beautiful modern strains would not tempt one to repeat the experiment. Mendelian principles drive home the lesson that we must expect nothing in the way of novelty from the first generation, and that it is on the next one that we must concentrate our attention.

Another extremely valuable instrument which Mendel has put into the hand of the breeder is the power of effecting the combination in one strain of desirable characters existing in separate strains, with great swiftness and precision. If the two characters we wish to combine are recessive ones, our task is an easy one, for we can effect their association in 1 individual in every 16 in the F_2 generation. If, however, it is a dominant one, we have to wait till the following generation in order to determine which of the individuals bearing the dominant character are heterozygotes and which homozygotes.

[1] Royal Horticultural Society, Third International Conference on Genetics, p. 376.

But we need not multiply instances. It must be sufficiently evident that the value of the tool which has been put into the hand of the breeder of animals and plants is such as can hardly yet be estimated.

Having now given examples of some of the more salient hereditary phenomena with which the Mendelian has been able to deal, and having made a brief reference to the economical importance of by-products of Mendelian investigation, we must resume the thread of the story and see whither it leads.

The culmination of Mendelian speculation is reached in the suggestion—for it is no more than that at present—that the inheritance of sex is Mendelian; that "male" and "female" represent two allelomorphs of a pair, like yellowness and greenness in peas. Sex certainly is a character which is inherited alternatively—that is to say, in organisms in which bi-sexual reproduction is the rule, an individual exhibits either the character of one parent or that of the other, but never a mixture of the two. It is a male or a female. But there is one profound difference between the hereditary relationship of the two characters, male and female, and the two characters, yellow and green; in the latter, one character (yellow) is always dominant over the other (green), in the former this is manifestly not the case. Let us see how the matter is dealt with by Castle, who was the first to formulate definitely the theory of the Mendelian inheritance of sex in a paper entitled "The Heredity of Sex."[1] He supposes that half of the germ-cells of every male individual contain the unit character "male," and half the unit character "female"; and that the germ-cells of females are similarly constituted. Now the result of the random union of a number of such spermatozoa and ova would be: 25 per cent. ♂♂ zygotes, 50 per cent. ♀♂, and 25 per cent. ♀♀. We see at once that all the homozygotes—the ♂♂s and the ♀♀s—if they mated with their like (*i.e.* ♀♀ with ♀♀, or ♂♂ with ♂♂) would be capable of producing members *of one sex only*. This difficulty is got over by saying that there is no reason to suppose that such sexually pure—*i.e.* homozygous—individuals exist; and Prof. Castle quotes cases which tend to show that in reality every organism, whether male or female, is potentially a hermaphrodite, and

[1] *Mus. Comp. Zool. Harvard*, vol. xl. No. 4.

possesses the characters of the opposite sex to that to which it belongs in a latent state. It is well known that the removal of the generative organs of an animal often results in the appearance of the secondary sexual characters of the opposite sex; for example, the human eunuch has the high voice and the distribution of fatty tissue which is characteristic of the female. The most decisive case is perhaps that which is cited by Darwin, who relates that when the domestic cock is crossed with the hen pheasant, those of the offspring which happen to be males *have the secondary sexual characters of the male pheasant.* There is abundant evidence over and above that cited by Prof. Castle for the truth of his contention that members of both sexes are potentially hermaphrodite. So high an authority as Dr. Curt Herbst concludes, after an exhaustive scrutiny of a great mass of evidence, first, that the presence in a functional state of the generative organs of a particular sex are an indispensable condition for the elaboration of the secondary sexual characters of that sex; and, secondly, that the functional generative organ exercises a negative influence—*preventing the appearance of the secondary sexual characters of the opposite sex.*[1]

To return therefore to our argument, Prof. Castle contends—and we have seen that his contention is supported by independent evidence—that the individuals of a normally bi-sexual species are not sexually pure, but potentially hermaphrodite. From this he concludes that the homozygous individuals ♀♀ and ♂♂ do not exist. He accounts for their non-existence by supposing that the unions between ova and spermatozoa bearing similar sexes are infertile. And he adduces evidence, other than the *a priori* considerations we have spoken of, for this view. Only half the unions are fertile, namely, those between ova and spermatozoa bearing dissimilar sexes. And, in this way, only heterozygotes are produced which are capable of producing both sexes. Furthermore, to account for the fact that half of these heterozygotes display the male and half the female primary and sexual characters, we have to suppose that in the case of the allelomorphic pair "male" and "female," we have alternative dominance of either of the pair, as opposed to the fixed dominance of one of the pair which obtains in the ordinary Mendelian pairs.

We have stated that it has been shown that the characters

[1] *Formative Reize*, p. 76.

of one sex are latent in the opposite one. It is extremely important that this phrase "latent" should not be misconstrued. The manner in which the Mendelians picture this "latency" is to suppose that the members of both sexes are heterozygotes, half of whose germ-cells contain the male character and half the female character. They do *not* believe—and this is the essence of the Mendelian doctrine of gametic purity—that the germ-cell which produces a female contains maleness as well. Maleness would only be admitted to be latent in a woman in the sense that greenness is latent in a cross between a green and a yellow pea.

There is another sense in which the phrase *latent* is used which must not be misconstrued. When a grey-skinned was crossed with a white-skinned pea, the hybrids, which were grey with purple spots, gave in F_2 nine greys with purple spots, three greys, and four whites, three of which, it will be remembered, were whites carrying purple spots, which was not manifested because the grey was absent. Bateson protests against the statement that purple spot is latent in these whites. "Sulphate of copper," he says, "is blue and chloride of copper is green, but it would be improper to speak of blue as latent in sulphuric acid, or of green as latent in hydrochloric acid."

STEREO-CHEMISTRY AND ITS APPLICATION TO PHYSIOLOGY

By A. W. STEWART, D.Sc.

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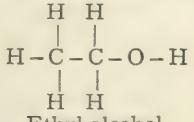
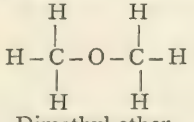
When two substances contain equal numbers of similar atoms and yet show a difference in properties, they are said to be *isomeric*. In the great majority of cases isomerism is due to a structural difference between the compounds in question—*i.e.* though they are made up of the same atoms, these atoms are differently linked together in the several substances. For example, ethyl alcohol and dimethyl ether are isomeric substances, both having the formula C_2H_6O; in the alcohol the oxygen atom links together a carbon and a hydrogen atom, while in the other case it unites two carbon atoms:



Ethyl alcohol.



Dimethyl ether.

This view sufficed for a time to explain nearly every case of isomerism known; but as more and more facts were accumulated, it became evident that some examples of isomerism could not be classed under this head; for compounds were discovered whose structures were identical, while very considerable differences were found in their properties. Organic chemists were thus forced to seek some new explanation for these phenomena.

It had been observed that several of these isomeric bodies had a peculiar influence on polarised light. It is well known that many crystalline compounds in the solid state will rotate the plane of polarisation of a polarised ray passed through the crystal; but since quartz possesses this property only when crystalline and not when in a state of amorphous silica, it is evident that we are dealing in its case merely with the influence of crystalline structure. But if we take a crystal of natural tartaric acid and dissolve it in water, so that it loses its crystalline form, we find that in solution it still has an action on the plane of polarisation. Similarly, camphor when fused does not

lose its power of rotating the plane of polarisation, though it has lost its crystalline character. In the case of these substances, therefore, the influence exerted on the plane of polarisation is not due to the same cause as the rotatory power of the quartz crystal; it is rather to be sought within the molecule itself.

It has been found that these *optically active* substances exist in two forms, one of which rotates the plane of polarisation to the right, while the other isomer rotates it through an equal angle to the left. This has led to the adoption of the current nomenclature, wherein the name of the right-rotating substance has the prefix *dextro* attached to it, while the left-rotating variety is termed the *lævo* isomer. Thus we speak of dextro camphor and lævo camphor, or simply *d*-camphor and *l*-camphor. Now, obviously, if we mix equal quantities of the dextro and lævo isomer together, the resulting mixture will have no influence upon the plane of polarisation; for the action of the right-rotating substance will be counteracted by the equal and opposite effect of the left-rotating body, and the mixture will be inactive by *external compensation*. Under certain conditions it is found that the dextro and lævo molecules combine together to form an actual inactive compound, which is termed a *racemic* substance (usually abbreviated to *r* or *dl*). The dextro and lævo bodies are usually referred to as *optical antipodes*.

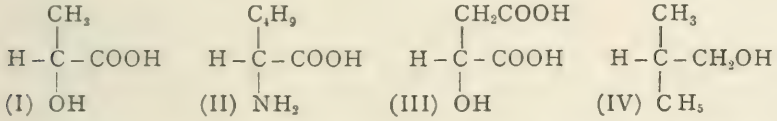
The first successful step in the study of these substances was made by Pasteur[1] in 1860, when he took up the question of the tartaric acids. He was able to show how to isolate the dextro and lævo forms from the racemic compound, discovering no fewer than three separate methods of attaining this object. He showed, further, that while racemic tartaric acid crystallizes in holohedral form, the dextro and lævo isomers show hemihedrism, the dextro crystal being the mirror image of the lævo crystal. A consideration of the general properties of the three substances led Pasteur to the conclusion that in the dextro and lævo forms the atoms might be grouped in spirals or situated at the corners of a tetrahedron. He was unable, in the elementary state of structural chemistry at that time, to do more than put this forward as a conjecture, and it remained for two later investigators to reap the credit of establishing the correctness of this view.

[1] Pasteur, *Recherches sur la dissymmétrie moléculaire des produits organiques naturels*, Paris (1860).

Pasteur believed that the optical activity of the tartaric acids was undoubtedly due to an asymmetric arrangement of which the right-handed form was the mirror image of the left-handed variety, just as a left-handed glove is a mirror image of a right-handed one. Thirteen years later, in 1873, Wislicenus[1] proved that active and inactive lactic acids had identical structures; and in his examination of the question, added, "The facts force us to explain the difference between isomeric molecules of similar structure by a different arrangement of atoms in space."

These suggestions did not lie dormant for any length of time, for in 1874 Le Bel[2] and van't Hoff[3] simultaneously and independently published papers in which the foundations of modern stereo-chemistry were laid.

Le Bel and van't Hoff pointed out that in every case where optical activity had been observed in a compound, the substance in question contained at least one *asymmetric carbon atom*, *i.e.* an atom whose four valencies are attached to four different radicles. Examples of this are found in lactic acid (i), leucine (ii), malic acid (iii), and amyl alcohol (iv), represented in the formulæ below, in each of which the asymmetric atom lies in the centre of the figure:



In the literature of that time, some cases were recorded which conflicted with this view, but later work has proved them to be due to faulty observation, and the Le Bel-van't Hoff theory has been completely established.

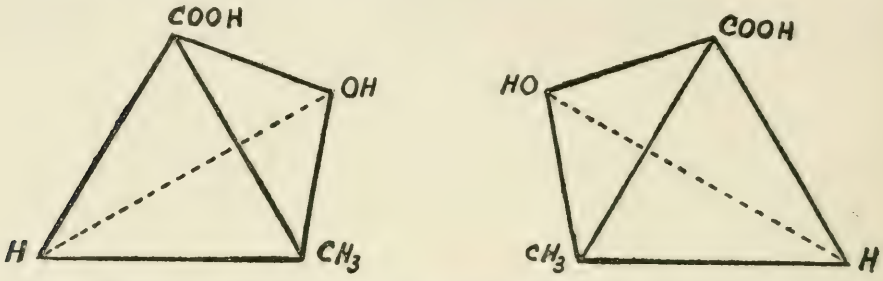
Now, when we examine the problem in the light of this evidence, we find that it is at once simplified. We need only recur to Pasteur's view that atoms in optically active compounds are situated at the corners of a tetrahedron, and the matter becomes clear. Let us suppose that the asymmetric carbon atom is situated at the centre of a tetrahedon, and that the four groups attached to it lie at the tetrahedron's corners. In the

[1] Wislicenus, *Annalen der Chem.*, **167**, 343 (1873).

[2] Le Bel, *Bull. Soc. chim.*, [2], **22**, 377 (1874).

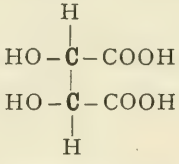
[3] van't Hoff, *Voorstell tot uitbreidung der structuur-formules in de ruimte*, Utrecht (1874).

case of lactic acid, we shall find that two arrangements are possible, each being the mirror image of the other:

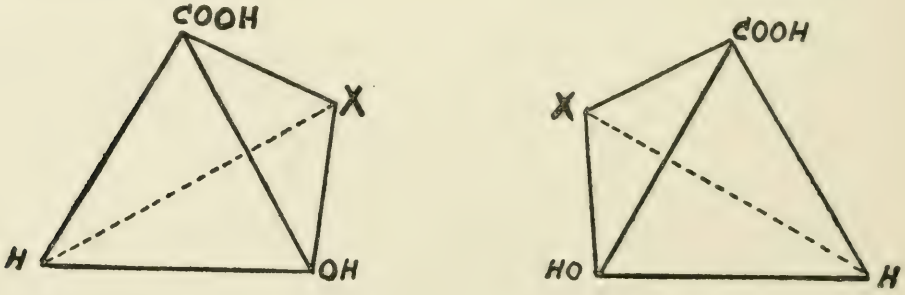


The two arrangements are unsuperposable, being related to one another as a man's right hand is to his left. Compounds which owe their difference to an arrangement of atoms in space are said to be *stereo-isomeric.*

We must now consider the effect of introducing a second asymmetric carbon atom into the molecule. This was first observed by Pasteur in the researches on the tartaric acids which we have already mentioned. When we examine the structural formula of tartaric acid, we see that it contains two asymmetric carbon atoms, which are marked by heavy type:



If we divide the molecule into two halves of identical structure, —CH(OH)COOH, we shall see that we have now three possible arrangements of atoms in space instead of two, as in the case of lactic acid. Instead of the second group —CH(OH)COOH, let us write X for convenience; then, as before, we have the two arrangements:



These represent the right-handed and left-handed varieties of the grouping XCH(OH)COOH. But now we have still to consider the grouping of the atoms in X, the second radicle $-CH(OH)COOH$. They also may be arranged in dextro or lævo form. Now, if we call the rotatory power of the one half of the molecule A, we shall find that first we have

$$+A \qquad\qquad -A$$

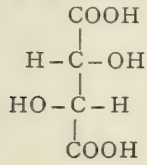
and to each of these we can add a dextro or a lævo form of the second half molecule, which has also a rotatory power, A. This gives us :

+ A	+ A	− A	− A
+ A	− A	+ A	− A
(1)	(2)	(3)	(4)

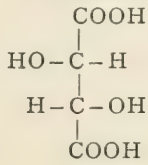
Form (1) and form (4) represent dextro- and lævo-rotatory bodies having a rotatory power of twice A. But when we examine forms (2) and (3) we find that they have no rotatory power at all, one half of the molecule counteracting the other half, just as one molecule of dextro camphor counteracts a molecule of lævo camphor and produces inactive camphor. The inactivity of the tartaric acid, however, depends upon compensation *within* the molecule itself, and the case is said to be one of *internal compensation*. Internally compensated tartaric acid is usually termed meso-tartaric for the sake of convenience.

The tetrahedron symbol does not lend itself to easy reproduction in printing, so other means were devised to represent the *configuration* or space arrangement of atoms in a compound. The method suggested by Fischer is the most generally employed. To use his formulæ, the tetrahedron model is first supposed to be built up in the usual way ; and then, after laying it on a sheet of paper, the positions of the groups of atoms are projected upon the paper. As an example, we may draw the formulæ of the tartaric acids, giving a sketch of the tetrahedra in each case :

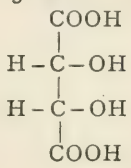
1. d-tartaric.

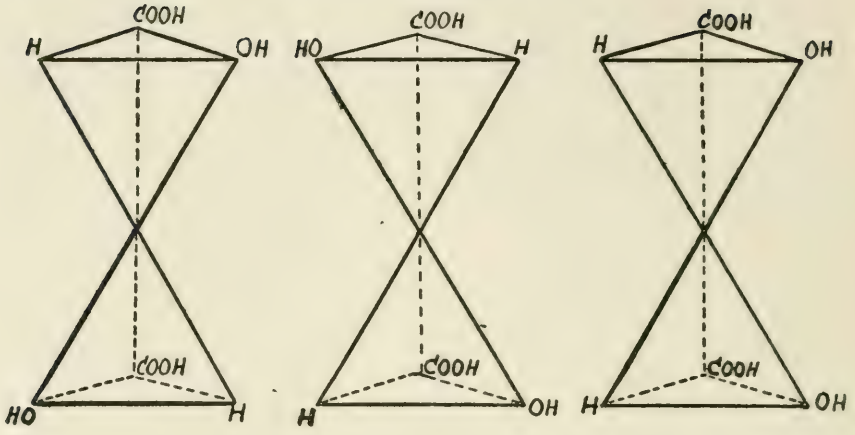


2. l-tartaric.



3. Meso-tartaric.





An equimolecular mixture of (1) and (2) gives racemic acid.

One point must be noticed. It might be supposed that if the two tetrahedra in the tartaric acids were to revolve around their common axis, the one isomer could be changed into the other, but this is not the case. The configuration of each isomer depends upon the *order* in which the groups are disposed around the tetrahedron, and no revolution can alter this. It is assumed that in these isomers the two tetrahedra can vibrate quite freely; but it is probable that, under any given conditions, they will come into equilibrium in some fixed position.

In order to make clear the difference in physical properties which is produced by differences in the arrangements of atoms in space, a table is given below which includes the chief properties of the dextro, lævo, meso, and racemic forms of tartaric acid:

	Dextro	Lævo	Meso	Racemic
Melting-point . . .	170°	170°	140°	206°
Affinity Constant . .	0·097	0·097	0·060	0·097 [1]
Specific Gravity . .	1·755	1·754	1·666	1·697
Solubility in 100 parts of water at 15° .	136	136	125	17

We must now consider what influence is exerted upon certain reactions by these differences in the spacial arrangements of atoms; and, in the first instance, we may deal with the actions of certain organisms upon racemic compounds. It has been observed by many observers,[2] of whom Pasteur was the pioneer,

[1] This is probably not the true value for K, as in dilute solutions the racemic acid dissociates into the two antipodes.

[2] A complete set of references is to be found in Landolt's *Optische Drehungsvermögen organischer Substanzen*, Brunswick; or in Winther, *Ber.*, **28**, 3022 (1895).

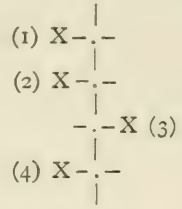
that when moulds of various kinds were allowed to grow in a racemic solution to which phosphates or other nutrient matter had been added, the solution gradually became optically active, an excess of one antipode being left. At one time it was supposed that the mould was unable to attack one antipode, and confined its destructive action entirely to the other; but the work of Frankland and MacGregor,[1] and of McKenzie and Harden,[2] showed this to be erroneous; the mould destroys both antipodes, but appears able to attack one much more rapidly than the other. This difference in action must be due to the difference in configuration between the two antipodes. Since some moulds attack the dextro form while others destroy the lævo variety, it is possible to obtain either the left-handed or the right-handed form of a given body by utilising the appropriate mould. For instance, if we treat racemic mandelic acid with the ferment *Penicillium glaucum*, an excess of dextro mandelic acid results, while lævo mandelic is obtained by using *Saccharomyces ellipsoideus*. Fischer and Thierfelder[3] have noted another case of fermentation in which even a slight difference in the configuration appears to exert a considerable influence. They treated the dextro forms of glucose, mannose, galactose, and talose with the same ferment, and found that while the first two were easily fermented, galactose was attacked only with difficulty, while talose was not affected. When we examine the space formulæ of the four bodies, a possible explanation of this difference suggests itself:

Glucose	Mannose	Galactose	Talose
CH_2OH	CH_2OH	CH_2OH	CH_2OH
HO.H	HO.H	HO.H	HO.H
HO.H	HO.H	H.OH	H.OH
H.OH	H.OH	H.OH	H.OH
HO.H	H.OH	HO.H	H.OH
CHO	CHO	CHO	CHO
Ferment easily.		Fermentable	Unfermented.

Fischer supposes that ferment and sugar fit each other as a key fits a lock; so that certain "wards" of the sugar can be reached only if the ferment has a configuration similar to that of the sugar. If we symbolise part of the ferment in the same way as the sugars, and represent by X the "ward" which

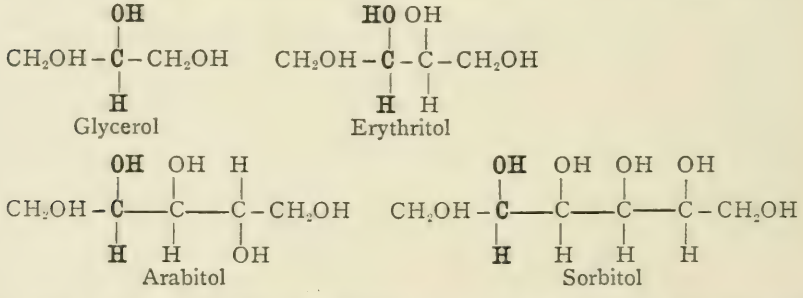
[1] Frankland and MacGregor, *Journ. Chem. Soc.*, **63**, 1034 (1893).
[2] McKenzie and Harden, *Journ. Chem. Soc.*, **83**, 424 (1903).
[3] Fischer and Thierfelder, *Ber.*, **27**, 2035 (1894).

touches the hydroxyl group, we shall find that a perfect ferment for glucose would be written thus:

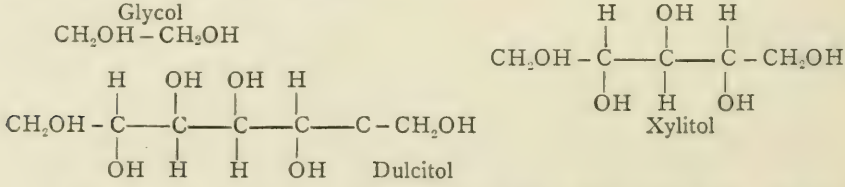


Here four "wards" come into play. In the case of mannose, only three of these (1, 2, and 3) are able to act; in galactose, 1, 3, and 4 would be able to attack the hydroxyl groups; while in talose only two (1 and 3) could come into action. This analogy of lock and key must not be pushed too far, but there certainly appears to be some relation between configuration and the ease of fermentation in certain compounds.

Results of a somewhat similar nature were obtained by Bertrand[1] in his studies of the action of the sorbose bacterium (*Bacterium xylinum*) on certain polyhydric alcohols. He found that the following alcohols were attacked by the organism:

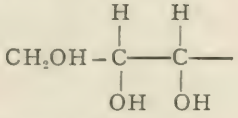


In each case the heavily printed —CHOH— group is oxidised to —CO—. On the other hand, compounds such as those below are not attacked in this way:



[1] Bertrand, *Bull. Soc. chim.*, [3], **19**, 347, 947, 999 (1898); *Compt. rend.*, **122**, 900 (1896); **126**, 762 (1898).

An examination of the formulæ shows that two conditions must be fulfilled before the bacterium can act. In the first place, the compound must contain a $-CHOH-$ group next a $-CH_2OH$ radicle; and in the second place it must have a space formula in which two hydroxyl groups lie next each other. In other words, unless a compound contains the grouping



the bacterium cannot attack it.

The foregoing results are sufficient to show that an intimate relationship exists between the molecular configuration of substances and the readiness with which they are attacked by ferments. Since this relationship exists in these cases, it is natural to expect a similar connection between the configuration of a substance and its behaviour in the bodies of animals. A large portion of all animal tissue is made up of asymmetric substances, and it appears probable that if an optically active substance is digested, either the dextro or the lævo form will react more easily than its antipode during the process.

When we consider the case of the three possible substances (dextro, lævo, and racemic), and suppose for the sake of convenience that the dextro compound is more easily assimilated than the lævo antipode by a given animal, we shall find that the racemic substance offers a further peculiarity; for if it passes through the organism as an actual compound, both its components will be destroyed to a certain equal extent, while if it breaks down into a mixture of its constituents, the dextro form will be attacked while the lævo isomer is unassimilated. We shall meet several cases of this kind in the work about to be described.

The simplest case of all is that of the tartaric acids, which has been described by Brion.[1] He found that racemic acid was least oxidised in the animal organism; next came dextro tartaric; while lævo tartaric and meso tartaric were most strongly acted upon. This proves that racemic acid, in its passage through the organism, is not decomposed into a mixture of dextro and lævo forms; as in that case the lævo form would be destroyed and a preponderance of the dextro form would remain.

[1] Brion, *Zeit. physiol. Chem.*, **25**, 283 (1898).

Further, the action of the animal tissue upon meso tartaric acid can be explained in the following way. Lævo tartaric acid is made up of two radicles, $-CH(OH)COOH$, each of which is arranged in lævo configuration, which is favourable to the attack of the organism; dextro tartaric acid, being composed of two dextro radicles, $-CH(OH)COOH$, offers no foothold to the destructive action of the tissues: the meso form, being made up of one dextro and one lævo radicle, offers a point of attack in the latter, and is therefore destroyed.

Neuberg and his collaborators have studied that part of the subject, and have examined several cases of the behaviour of optical antipodes in the animal body. We may take the case of the three arabinoses and their derivatives[1] as an example. The arabinoses are sugars which have the structural formula $CH_2OH \cdot CH(OH) \cdot CH(OH) \cdot CH(OH) \cdot CHO$. The figures below represent in each case the percentage of substance recovered from the urine of rabbits; column I shows the results obtained when the substance was introduced through the mouth, II when introduced subcutaneously, and III intravenously:

	I	II	III
Lævo arabinose	14·5	7·1	28·3
Racemic arabinose . . .	28·5	31·7	29·0
Dextro arabinose . . .	31·2	36·0	31·0

These results agree fairly well among themselves, when allowance is made for inevitable experimental differences. The three sodium salts of the arabonic acids, $CH_2OH.CHOH.CHOH.CHOH.COONa$, show similar differences, though less of the lævo than of the dextro isomer is attacked in their case. When the corresponding alcohol arabite, $CH_2OH.CHOH.CHOH.CHOH.CH_2OH$, was supplied to the animal, small quantities of pentoses, $CH_2OH.CHOH.CHOH.CHOH.CHO$, appeared in the urine, one of the $-CH_2OH$ groups at the end of the chain being oxidised to $-CHO$.

A very important result was obtained by Neuberg and Wohlgemuth[2] in the case of the human subject. In certain diseases it is found that *racemic* arabinose makes its appearance. Now, when racemic arabinose was supplied to a human subject it was found that a considerable action took place, about two-

[1] Neuberg and Wohlgemuth, *Ber.*, **34**, 1745 (1901); *Zeit. physiol. Chem.*, **35**, 41 (1902).

[2] *Ibid.*

thirds of the excreted substance being in the active form, showing that the racemic compound had been broken down into its components, one of which had been destroyed. It is therefore obvious that the formation of racemic arabinose due to disease must take place in an organ so situated that after being liberated from it the arabinose is not submitted to any process of decomposition.

Since a difference in configuration such as exists between dextro and lævo forms has such a pronounced effect on the nutritive values of the arabinoses, it is not astonishing that Nagano[1] found a similar difference between lævo xylose and lævo arabinose, the former being more easily absorbed by the system. The configuration formulæ of the two substances are shown below.

l-Xylose	l-Arabinose
CH_2OH	CH_2OH
HO.H	H.OH
H.OH	H.OH
HO.H	HO.H
CHO	CHO

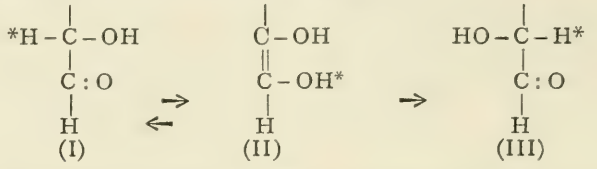
Analogous results were obtained by Neuberg and Meyer[2] in the case of the mannoses, $CH_2OH.CHOH.CHOH.CHOH.CHOH.CHO$. The dextro form appeared best suited for nutrition. The chief point of interest for stereochemical workers lies in the fact that during the passage of the dextro, lævo and racemic compounds through the system derivatives of another sugar, glucose, were produced. The formulæ of *d*-mannose and *d*-glucose are given below.

d-Mannose	d-Glucose
CH_2OH	CH_2OH
HO.H	HO.H
HO.H	HO.H
H.OH	H.OH
H.OH	HO.H
CHO	CHO

As can be seen from an inspection of the formulæ, the change from mannose to glucose necessitates the rearrangement of the groups attached to the lowest carbon atom in the formula. Such a rearrangement implies no great strain on the molecular

[1] Nagano, *Pfluger's Archiv.*, **90**, 389 (1902).
[2] Neuberg and Meyer, *Zeit. physiol. Chem.*, **37**, 530 (1903).

arrangement, however, as it may be supposed to take place by a wandering of a hydrogen atom, thus:



The form (II) is unstable. If a model be built up to represent these formulæ, it will be found that when the hydrogen atom marked with an asterisk wanders back from the position (II) it may give rise to (I) or (III). The action of the living tissues in this case is evidently exerted to bring about the change of (II) into (III) rather than of (II) into (I).

Rosenfeld[1] has found that sugars are excreted in urine in the following order: galactose, mannose, glucose; while the alcohols follow the analogous order: dulcite, mannite, sorbite.

CH_2OH	CH_2OH	CH_2OH
HO.H	HO.H	HO.H
H.OH	HO.H	HO.H
H.OH	H.OH	H.OH
HO.H	H.OH	HO.H
CHO	CHO	CHO
d-Galactose	**d**-Mannose	**d**-Glucose
CH_2OH	CH_2OH	CH_2OH
HO.H	HO.H	HO.H
H.OH	HO.H	HO.H
H.OH	H.OH	H.OH
HO.H	H.OH	HO.H
CH OH	CH_2OH	CH_2OH
Dulcite	**d**-Mannite	**d**-Sorbite

These differences are probably caused by some reaction similar to that which takes place in the selective fermentation of certain sugars, which was described above.

Having dealt with the influence of the configuration of optical antipodes upon their nutritive value, we must turn to another side of the question, and consider the relations between the configurations and toxic qualities of various compounds. No general rule can be laid down in this case; for, as in the case of nutrition, the experimental data are here far too meagre to allow of generalisation.

In the first place, it is advisable to point out that in some cases influences intervene which are only indirectly

[1] Rosenfeld, *Centralblatt f. innere Medicin*, **21**, 177 (1900).

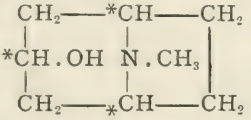
attributable to the special arrangement of atoms in space. For example, Chabrié's [1] experiments on the toxicity of the tartaric acids are stated by Cushny [2] to be unreliable, as the toxic agent in this case is really the hydrogen ion and not the acid itself. Since the configuration of the acid's molecule influences its ionization, however, it should in this way have some effect upon its toxicity also. Chabrié's figures are given below. They are calculated in the following way: Let x be the weight of acid required to kill one kilogram weight of an animal in one minute, p the weight of acid, and w the weight of animal used, and T the time required to kill determined by experiment, then

$$\frac{1}{x} = \frac{1000\,p.\,T}{w}$$

	$\frac{1}{x}$
Lævo-tartaric acid	34·26
Dextro „ „	104·24
Racemic acid	165·25
Meso-tartaric acid	196·27

It will be seen from a comparison of these figures with Brion's results, which have already been given, that there is no close connection between the two.

Owing to their applications in medicine, the behaviour of compounds of the tropine series has been investigated with more thoroughness than that of other isomers. The parent substance, tropine, has the following structure:



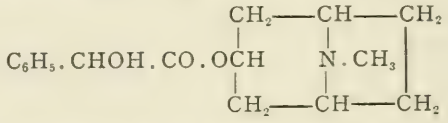
in which three carbon atoms (marked with asterisks) are asymmetric. When tropine (melting-point 62°) is treated with sodium amylate, it is converted into an isomeric body, pseudo-tropine (melting-point 108°). The two compounds have exactly the same structure, and the difference between them is due, probably, to the different arrangement of the —CH.OH— radicle. Ladenburg [3] has suggested another explanation depending

[1] Chabrié, *Compt. rend.*, **116**, 1410 (1893).

[2] Cushny, *Journ. Physiol.*, **30**, 193 (1904).

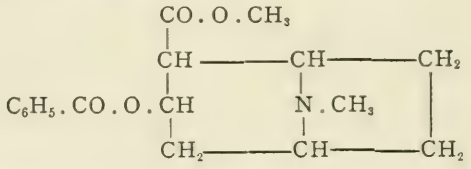
[3] See Ladenburg, *Ber.*, **26**, 854 (1893); **36**, 3694 (1903); and Groschuff, *ibid.* **34**, 2974 (1904), for examples of this type of isomerism.

upon the special arrangement of groups around the nitrogen atom. Whichever be true, the isomerism of tropine and pseudo-tropine is caused by spacial, not structural differences. When we allow mandelic acid to react with the hydroxyl groups of tropine and pseudo-tropine, we obtain two stereoisomeric esters, each of which has the following structure:



The ester obtained from tropine has a strong mydriatic action, while the pseudo-tropine ester has no such property.

When one of the hydrogen atoms of tropine is replaced by a carboxyl group, tropine carboxylic acid is formed; and when the alcoholic radicle of this substance is benzoylated while the carboxyl group is esterified with methyl alcohol, we obtain cocaine, which has the formula:



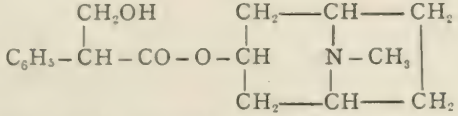
Cocaine, of course, since it contains asymmetric carbon atoms, exists in two antipodic forms of equal and opposite rotatory power. Poulsson[1] found that when applied to the tongue the paralysing action of dextro cocaine was stronger and set in more rapidly than that of lævo cocaine; but, on the other hand, the effect of the dextro isomer was not so permanent as that of the lævo antipode. In the case of rabbits and frogs, the fatal dose seems to be the same for each isomer. Ehrlich and Einhorn[2] arrived at similar results.

According to Hesse[3] the therapeutic action of scopolamine and the corresponding racemic compound atroscine differ from each other.

Cushny[4] investigated the actions of lævo hyoscyamine and

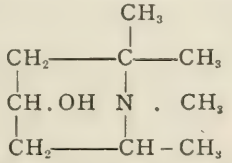
[1] Poulsson, *Archiv exp. Path. u. Pharmak.*, **27**, 309 (1890).
[2] Ehrlich and Einhorn, *Ber.*, **27**, 1870 (1894).
[3] Hesse, *Journ. f. prakt. Chem.*, [2], **64**, 353 (1901).
[4] Cushny, *Journ. Physiol.*, **30**, 193 (1904).

atropine, which is the corresponding racemic form. These substances are the tropine esters of tropic acid, and have the formula :



It was found that both atropine and *l*-hyoscyamine have the same action upon the central nervous system of animals, the effects being not only qualitatively but also quantitively identical. When applied to the spinal cord reflexes, atropine has a more powerful influence than the active isomer; but this is reversed in the case of the nerve ends of the heart, the salivary glands, or the pupil of the eye; for at each of these points *l*-hyoscyamine has an effect twice as strong as that produced by atropine. From these results we can deduce the probable effect of dextro hyoscyamine. We know that *dl*-hyoscyamine (atropine) has the same effect as *l*-hyoscyamine on the central nerve system of mammals; therefore in this case dextro and lævo forms must have the same effect. But, on the other hand, *dl*-hyoscyamine has a greater effect than *l*-hyoscyamine on the spinal cord, so that here, obviously, *d*-hyoscyamine must exert a greater influence than *l*-hyoscyamine. In the case of the heart, the effects are again reversed; for since here *l*-hyoscyamine is twice as powerful as *dl*-hyoscyamine, it appears that *d*-hyoscyamine must be almost without action. Of course this reasoning assumes that the racemic compound breaks up into the two antipodes before it acts upon the various nerves.

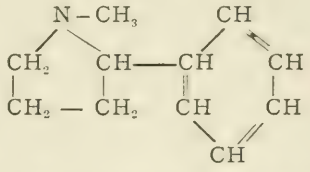
Two other substances should now be described, as they are closely allied to the tropine group. If in the formula of tropine we replace the radicle $—CH_2—CH_2—$ by two methyl groups, and at the same time exchange a hydrogen atom of a —CH— group for a methyl radicle, we obtain the following formula, which represents N-methyl-vinyl-diacetone-alkamine :



Like tropine itself, this compound exists in two isomeric forms, the mandelic ester of one being an oil, while that of the other

melts at 113°. In this case also the labile isomer has a mydriatic action, while the stable isomer lacks that property.

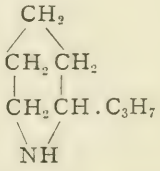
The actions of the two antipodic forms of nicotine:



have been studied by Mayor,[1] who found that the lævo form was twice as poisonous as the other. When dextro nicotine is injected no pain is felt; but when the lævo isomer is used it produces both pain and excitation. The lævo antipode causes paralysis, spasms, retardation of the action of the heart, and finally death by the cessation of respiration; while the same dose of dextro nicotine produces only a strong shivering fit, which soon passes off.

Only one or two other cases need be mentioned in this division of the subject. Albertoni[2] has shown that the dextro-rotatory compound cinchonine differs in action from its lævo-rotatory isomer cinchonidine. The latter has a slower action, requires a larger dose to produce the same effect as the dextro isomer, gives rise to vomiting, and has a marked tendency to cause spasms. Quinine and quinidine, which are also optical isomers, differ in physiological action as well, quinidine having no narcotic properties, though showing the febrifugial action of quinine.

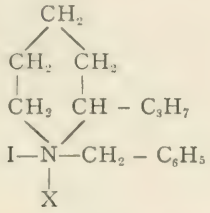
Coniine has the structure:



and as it contains an asymmetric carbon atom it occurs in active and racemic forms. Ladenburg and Falk[3] compared the action of the dextro antipode with that of the racemic form, but were unable to detect any difference between the two effects.

[1] Mayor, *Ber.*, **38**, 597 (1905).
[2] Albertoni, *Archiv exp. Path. u. Pharmak.*, **15**, 272.
[3] Ladenburg and Falk, *Annalen d. Chem.*, **247**, 83 (1888).

Hildebrandt,[1] examining ethyl, propyl, butyl, and iso-amyl derivatives of benzyl-coninium iodide, found that the isomer with the lower melting-point had also the weaker physiological action. The general formula of these bodies is shown below, the position occupied by the ethyl, propyl, butyl or iso-amyl radicle being indicated by X.

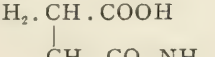


In the preceding paragraphs we have shown that the influence of the antipodic arrangement of atoms in a molecule is well marked in many cases. We must now mention several instances which show that differences in the configuration of compounds may affect our senses.

In the first place, we may point out that the crystals of the dextro and lævo forms of sodium ammonium tartrate differ from each other and from those of sodium ammonium racemate. The optically active compounds crystallise in hemihedral form, and the crystal of the *d*-salt is the mirror-image of that of the *l*-salt. The racemic salt exhibits no hemihedral faces in its crystals. The spacial arrangement of atoms, since it lies at the root of this difference in crystalline form, certainly affects our sense of touch.

Passing now to the question of smell, it has been found by Werner and Conrad[2] that the methyl esters of the two optically active trans-hexahydroterephthalic esters have different odours; while Schmidt and Tiemann observed that racemic terpenes were less odoriferous than the corresponding active isomers.

Taste, in its turn, is affected in the same way, for Piutti[3] states that the dextro and lævo forms of asparagine



[1] Hildebrandt, *Ber.*, **38**, 597 (1905).
[2] Werner and Conrad, *Ber.*, **32**, 3052 (1899).
[3] Piutti, *Compt. rend.*, **103**, 134 (1886).

have different tastes, that of the *d*-form being sweet, while the *l*-isomer is insipid. Fischer and Hagenbach[1] state that lævo leucine has a weakly bitter taste, while the dextro form is quite sweet. A similar difference was noted by Menozzi and Appiani,[2] who found that *d*-glutaminic acid was sweet, while the lævo form was tasteless. Glutaminic acid has the formula:

$$\begin{array}{l} NH_2 . CH . COOH \\ \quad\quad\quad | \\ \quad\quad CH_2 . CH_2 . COOH. \end{array}$$

In the case of mannose and glucose, the evidence is conflicting; for van Ekenstein[3] states that mannose is sweet, while glucose is bitter; but this is contradicted by Neuberg and Mayer.[4] Sternberg[5] considers that as a rule the difference between the isomers is quantitative rather than qualitative. If this be correct, it opens up a field for speculation. Marckwald and McKenzie[6] have shown that when an active acid is esterified with a racemic alcohol, or *vice versâ*, the reaction proceeds faster with one component of the racemic body than with the other. For instance, they found that lævo menthol combines more rapidly with dextro-mandelic acid than with lævo-mandelic acid. If we recall that Pasteur conceived nerves as asymmetric organisms, the "quantitative difference" between the actions of optical antipodes upon them might be explained by supposing that the asymmetric nerve substance combines more readily with one antipode than with the other. This, however, is pure theory.

The sense of sight is the last one with which we can deal. Byk[7] has shown that a solution of potassium copper racemate differs in tint from a solution of the potassium copper salt of dextro tartaric acid. Thus, the only sense which is not yet proved to be affected by optical isomerism is that of hearing. Even this might conceivably be influenced by differences in the rate of transmission of sound in active and racemic substances; but, so far, no experiments have been made in this direction.

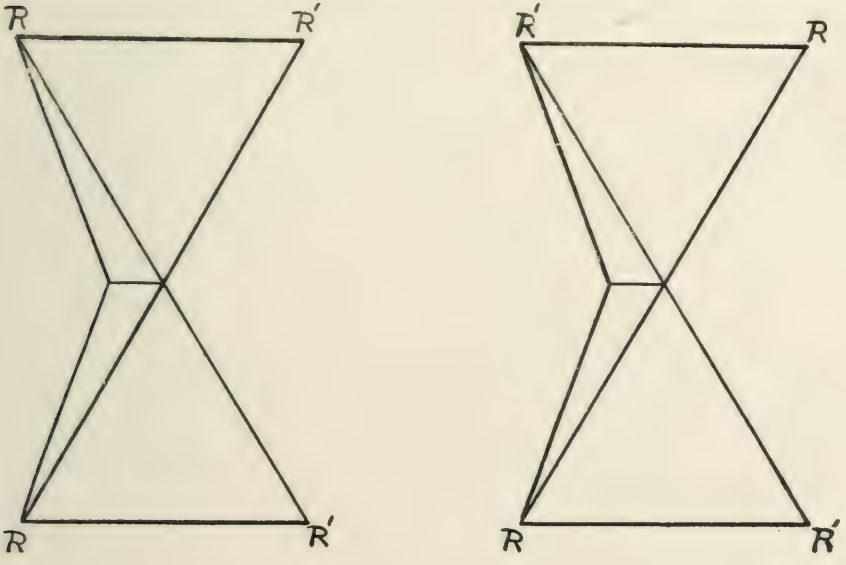
We must now turn to another branch of the subject, and

[1] Fischer and Hagenbach, *Ber.*, **34**, 3764 (1901).
[2] Menozzi and Appiani, *Atti R. Accad. Lincei* [5], **2**, II. 421 (1893).
[3] van Ekenstein, *Rec. trav. chim.*, **15**, 222 (1896).
[4] Neuberg and Mayer, *Zeit. physiol. Chem.*, **37**, 545 (1903).
[5] Sternberg, *Archiv Anat. u. Phys.*, **1898**, 457.
[6] Marckwald and McKenzie, *Ber.*, **32**, 2130 (1899).
[7] Byk, *Zeit. physikal. Chem.*, **59**, 682 (1904).

consider those cases in which isomerism is due to the presence in the molecule of a double bond between two atoms. The Le Bel-van't Hoff hypothesis assumed that, in the case of a single bond between two carbon atoms, free rotation of the atoms about their common axis was possible; but when we come to a compound containing a double bond, this postulate is abandoned, and it is assumed instead that the double linking of the atoms introduces an element of rigidity into the molecule. We may retain the tetrahedral symbols in this case also, and represent the compound ethylene, $CH_2{:}CH_2$, by two tetrahedra laid edge to edge instead of apex to apex, as in the previous cases.

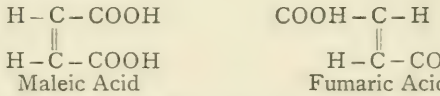
When we substitute other atoms or radicles for the four hydrogen atoms, we shall find that under certain conditions we have a system which is capable of two space arrangements. The necessary conditions are simple: we need only ensure that the two groups attached to each carbon atom differ from one another. In other words, isomerism is to be expected in its simplest form in the case of an ethylene derivative.

The two possible special arrangements are those represented by:



In the first case the two groups R lie one above the other, while in the second figure R lies over R^1; and since free rotation of the carbon atoms is assumed to be precluded by the nature of the double bond, no change in configuration can take place under ordinary conditions. It should be noted that all four groups RRR^1 R^1 lie in one plane, so that no isomerism due to enantiomorphous arrangements of atoms is to be expected, nor have these isomers any action upon the plane of polarisation. To distinguish these isomers from those containing asymmetric carbon atoms it is usual to term them *geometrical* isomers, as distinct from optical isomers.

The best known case of geometrical isomerism is that of maleic and fumaric acids, which may be represented thus, if the tetrahedra be omitted:



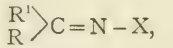
Maleic Acid — Fumaric Acid

When maleic acid is heated above its melting-point it is converted into fumaric acid. The former body is therefore termed the *labile* form, while fumaric acid is known as the *stable* isomer. As in the case of optical isomers, we may give here a table showing the principal physical properties of maleic and fumaric acids:

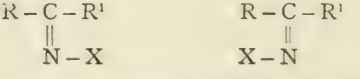
	Maleic Acid	Fumaric Acid
Melting-point	130°	Sublimes unchanged
Affinity Constant	1·17	0·093
Specific Gravity	1·590	1·625
Solubility in water	1:2	1:150

An extension of the hypothesis of geometrical isomerism to nitrogen compounds was made in 1890 by Hantzsch and Werner.[1] They assumed that when a carbon atom is doubly linked to a nitrogen atom, the third valency of the nitrogen atom is directed at a certain angle to the other two.

Now, in the compound



it is obvious that X may be supposed to lie near R or R^1; so that in this case also we have the possibility of two geometrical isomers:



[1] Hantzsch and Werner, *Ber.*, **23**, 11 (1890).

Much less attention has been given to the physiological action of stereoisomers of this type; and when we examine the literature of the subject, only a few scattered references are to be found. The cause of this apparent neglect is to be found in the fact that many of the alkaloids used in medicine occur in optically active forms, while geometrically isomeric bodies find no considerable place in pharmacology.

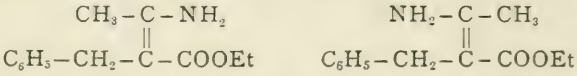
We may consider in the first place the effect which this type of isomerism exerts upon fungi. The fungus *Penicillium glaucum* grows readily in fumaric acid solution, but does not thrive in a solution of maleic acid. Using *Lupinus albus*, *L.*, Kahlenberg and True [1] established the following figures. When the fungus was introduced into a solution containing 116 grammes of acid in 6,400 litres of water, it died. When the solution was diluted to twice its volume, the organisms were able to live in the weakened solution. 116 grammes of maleic acid dissolved in 3,200 litres of water produced a solution incapable of supporting the organisms; but on dilution to 6,400 litres it became fit to support the fungi. These two sets of results, pointing as they do in opposite ways, seem to indicate a difference in the configurations of the active tissues of fungi, *Penicillium glaucum* being capable of attacking fumaric acid, *Lupinus albus*, *L.*, has a greater power of breaking down maleic acid. Further research in this field might lead to very interesting results, as the case seems to form a parallel to the action of ferments upon sugars, with which we have already dealt.

In toxic action the two acids differ considerably, as Ishizuka [2] has proved. In his experiments he took 1·94 grammes of acid for each kilogramme of dog used. This, in the case of maleic acid, proved a fatal dose; while with fumaric acid the same dose had no effect. It should be noted, however, that this result may be due to a difference in the strength of the two acids, maleic acid being much stronger than fumaric.

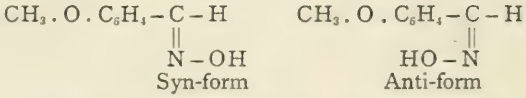
Toxic action, however, is not the only property in which these isomers show differences. It has been found that in the case of the compound benzyl-β-amidocrotonic ester, whose two configurations are shown below, the one form (M.P. 79°) is tasteless, while its isomer (M.P. 21°) has an intensely sweet

[1] Kahlenberg and True, *Botanical Gazette*, Chicago, **22**, 181 (1896).
[2] Ishizuka, *Bull. Coll. Agr.*, Tokyo, **2**, 484 (1897).

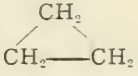
taste, which resembles that of peppermint. The two configurations are shown below:



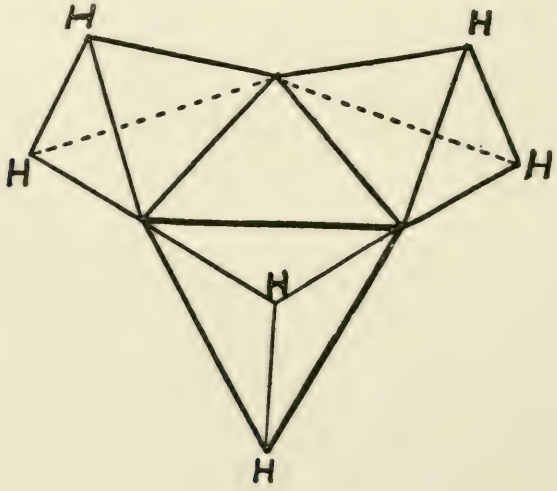
A similar example is found in the case of the aldoximes: the syn-form of anisaldoxime is tasteless, while the anti-form is very sweet:



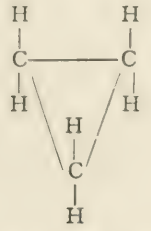
We must next deal with another case in which the rotation of carbon atoms about their axis of junction is hindered. When three or more carbon atoms are joined together to form a cyclic compound like trimethylene:



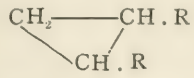
there are two important consequences. In the first place, free rotation of the carbon atoms becomes difficult, if not impossible. This can be seen by building up a model, when it will be found that a disruption of the molecule would almost certainly result from any such rearrangement. Secondly, the six hydrogen atoms in trimethylene are arranged in two parallel planes, the carbon atoms lying in a third plane between these two and parallel to them.



To save space in printing, this is usually written in the following way:

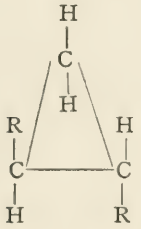


Now, here, as in the case we have already dealt with, substitution may produce a difference in configuration; for if we take the case of the compound:



we shall find that there are two possible spacial arrangements of the atoms. In the first, we have the two groups R and one hydrogen atom in the same plane, while in the second arrangement we have one R group and two hydrogen atoms in the same plane:

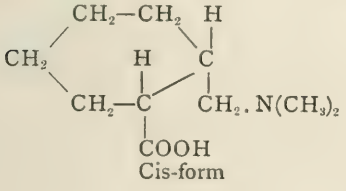
(I)



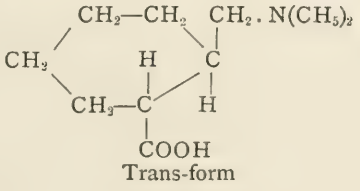
(II)

Form (I) is called the *cis*-form; form (II) the *trans*-form.

In this section two instances are known in which the two isomers differ from one another in smell. In each case the cis-form has a stupefying odour, while the trans-isomer is devoid of smell. The substances in question are the hexahydro-*o*-diethyl-benzylamine carboxylic acids:

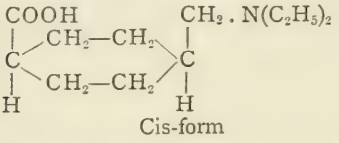


Cis-form

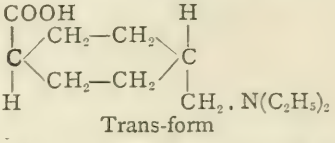


Trans-form

and the hexahydro-*p*-diethyl-benzylamine carboxylic acids :



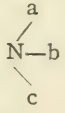
Cis-form



Trans-form

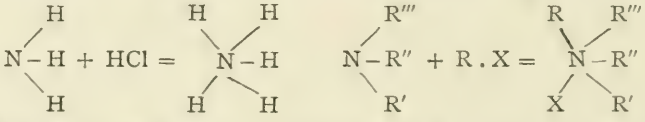
In the foregoing paragraphs we have been concerned with compounds which owed their isomerism to some peculiarity either in the arrangement of radicles round a carbon atom, or in the mode of linkage between atoms. We must now turn to the question of an isomerism which is due to other types of asymmetry, and we may commence with the case of asymmetric nitrogen.

Without going into any intricate details, it may be said that when a nitrogen atom is united to three separate radicles, no isomerism is to be found. Of course, as has been mentioned already, a nitrogen atom doubly linked to a carbon atom may give rise to isomerism, but in the case of a compound such as :



no isomerism occurs; and it is therefore assumed that the groups *a*, *b*, and *c* and the nitrogen atom lie in one plane or are free to move about in space. Were this not so, we should expect to find enantiomorphism shown by such substances.

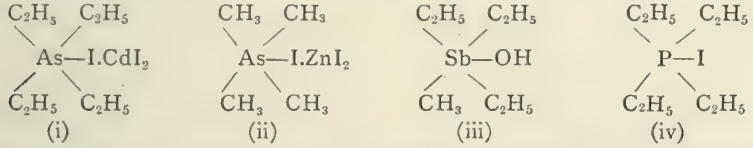
When we allow a tertiary amine to react with an alkyl halide, a quaternary ammonium salt is formed, just as ammonia and hydrochloric acid unite to form ammonium chloride :



When the four alkyl groups attached to the nitrogen atom differ from each other, as in the case of NRR′R″R‴Cl, the nitrogen atom is asymmetric, and the compound is found to exist in two enantiomorphous forms of equal and opposite rotatory power. The case is parallel to that of the asymmetric carbon atom. It is evident that in passing from the trivalent to the pentavalent condition of the nitrogen atom we have passed from a plane configuration of the molecule to one of three dimensions.

Crum Brown and Fraser[1] have shown that when certain alkaloids containing a trivalent nitrogen atom were allowed to unite with methyl iodide, they gained a new physiological property without losing any of their previous efficiency. The new attribute which they acquire by this increase in the valency of their nitrogen atoms resembles the chief property of curare, for these new methyl iodide addition products have a paralysing action upon the ends of the motor nerves of muscles. Curare itself contains a tertiary base curine, and Boehm[2] has shown that when curine is treated with methyl iodide it yields the quaternary salt curarine, which is two hundred and twenty-six times as poisonous as the mother substance, and which possesses in an acute degree the paralysing properties of curare. The most probable deduction from these results is that the change in the valency of the nitrogen atom lies at the root of these effects; but we must inquire further whether the consequent change in configuration has not some bearing upon the point.

Fortunately, the evidence on this subject is already at our disposal. We need only replace the nitrogen atom by some other atom which has the same faculty of changing from the trivalent to the pentavalent condition; and we are not hampered in our choice in this matter, as no fewer than three other elements have been examined by Vulpian.[3] The substances actually used by him were: (i) tetra-ethyl-arsonium cadmium iodide, (ii) tetra-methyl-arsonium cadmium iodide, (iii) methyl-triethyl-stibonium hydroxide, and (iv) tetra-ethyl-phosphonium iodide:



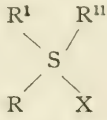
None of these substances shows the ordinary physiological behaviour of arsenic, antimony, or phosphorus, but all of them have a strong curare-like action, which seems to indicate that that action is due rather to the molecule as a whole than to the nature of the atom at the centre of the complex.

[1] Crum Brown and Fraser, *Trans. Roy. Soc. Edin.*, **25**, 707 (1868); *Proc. Roy. Soc. Edin.*, **1869**, 560.

[2] Boehm, *Archiv de Pharmacie*, **235**, 660 (1897); *Archiv exp. Path. u. Pharmak.* **6**, 101.

[3] Vulpian, *Archiv de phys. norm. et pathol.*, **1**, 472; see also Bürgi, *Archiv. exp. Path. u. Pharmak.*, **56**, 101.

Another example of the change of a plane molecular configuration into a three-dimensional one is to be found in the case of the sulphur compounds. In the alkyl sulphides having the formula $R - S - R'$, since there are only three groups, the configuration must be a plane one. When alkyl iodides are added to these sulphides, however, optically active substances can be produced, having the formula:



and from the occurrence of optical activity it is supposed that this case is analogous to that of the asymmetric carbon atom; and that the radicles R, R′, R″, and X are arranged at the corners of a tetrahedron whose centre is occupied by the sulphur atom. Of course this tetrahedral arrangement will persist even if the sulphur atom be not asymmetric and no optical activity is shown. Now Curci and Kunkel[1] have found that sulphine bases, such as trimethyl-sulphine hydroxide $(CH_3)_3 . S . OH$, have a pronounced curare-like action, which is lacking in the sulphides.

Similar results have been obtained by Bock[2] in his study of the ammonia derivatives of cobalt, chromium, and rhodium, which resemble each other to a certain extent. He was able to show that the nature of the metallic atom in the molecule had less influence upon the physiological action than was exerted by the general character of the compound.

Since in all these cases a change from a plane configuration to a three-dimensional one is taking place, and since, previously, we have shown how great an influence upon the physiological action of certain substances is exerted by a difference in the spacial arrangement of their atoms, we are not unjustified in assuming that the change in configuration has some influence in this case also.

Stereochemistry is usually divided into two sections. In the one division we deal with the purely static (or at least hypothetically static) relations between atoms which give rise to isomerism; in the second section we are concerned chiefly with

[1] Curci, *Archiv de pharm. et de therap.*, **4**, 1896; Kunkel, *Lehrbuch der Toxikologie* (1901).

[2] Bock, *Archiv exp. Path. u. Pharmak.*, **52**, 1, 30 (1904).

the dynamic effects of certain atomic groupings, their influence upon chemical reactions, and similar subjects. In the foregoing pages we have restricted our examination to the first division, and have dealt purely with the question of stereo-isomerism and its connection with the action of certain substances upon living tissues; but we must now turn to the second part of the subject and discuss the dynamic influence which certain atomic groups may exert upon others.

When we allow acetic acid to react with ethyl alcohol in presence of hydrochloric acid, water is eliminated and ethyl acetate is formed:

$$CH_3.COOH + C_2H_5OH = CH_3.COOC_2H_5 + H_2O$$

Now, the researches of Menschutkin, Sudborough, and Lloyd, and others[1] have shown that when substituted acetic acids are used instead of the parent substance, the amount of ester formed under given conditions decreases. The amount of this decrease depends on the complexity of the acid, and is quite independent of its strength, as the following table shows:

Formula of Acid.	Amount of Ester.	Affinity Constant.
$CH_3.COOH$	3·661	0·00180
$CH_3.CH_2.COOH$	3·044	0·00134
CH_3 \ $CH.COOH$ / CH_3	1·0196	0·00144
CH_3 \ $CH_3-C.COOH$ / CH_3	0·0909	0.000978

A similar decrease in the amount of ester formed is found if the complexity of the alcohol be increased while the acid remains the same throughout, *e.g.* when acetic acid is esterified with methyl, ethyl, isopropyl, and tertiary butyl alcohols. It is evident, therefore, that the cause of the decrease in the amount of ester formed is to be sought in the change in structure of the acid or of the alcohol. On examining the formulæ shown in the table, we find that in each case as we go down the table we are replacing a hydrogen atom by a methyl radicle. In other

[1] Menschutkin, *Ann. chim. phys.* [5], **20**, 229 (1880); **23**, 14 (1881); **30**, 81 (883); *Journ. Russ. Phys. Chem. Soc.* **9**, 316, 346 (1877); **10**, 278 (1878); **11**, 24, 345 (1879); **12**, 82 (1880); **13**, 564 (1881); **16**, 356 (1884); *Ber.* **13**, 1812 (1880); **30**, 278 1897); *Annalen der Chem.*, **195**, 334 (1879); **197**, 193 (1879); *Journ. Chem. Soc.*, **8** , 1532 (1906); Sudborough and Lloyd, *ibid.* **75**, 467 (1899).

words, we are exchanging a small light group for a large heavy one.

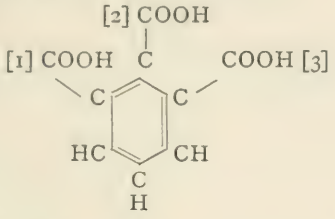
Let us next consider what takes place when an acid reacts with an alcohol. Obviously the hydroxyl group of the alcohol must come within range of the carboxyl group of the acid before any interaction can take place, since the ester is formed by the elimination of water between these two groups. Now, if we could interpose some barrier between these two radicles, we should hinder the esterification; and the more effective the barrier was, the less ester would be formed. Such a barrier is found in the methyl groups of the substituted acetic acids. According to the current theories of chemical action, a certain amount of free space must exist around the hydroxyl group before the carboxyl radicle can come within reaction range. The light hydrogen atoms are easily thrust aside by the heavy carboxyl group, and therefore the esterification of acetic acid is easy. When we replace the light hydrogen atoms by the heavier methyl radicles, however, the resistance to the entrance of the carboxyl radicle becomes serious, and consequently much less ester results. The following analogy may help to make the matter clearer: Suppose a man to be desirous of passing through a certain door, guarded by children. He would be able to pass because his strength would be greater than that of the guards; but if the children became men, his chances of passing the door would be very considerably reduced.

This *steric hindrance*, as it is called, has been observed in nearly every class of reaction; and there seems to be no doubt that the substitution of one group for another may influence the course of a reaction to an extent which seems quite out of comparison with the slight change in the chemical character of the substance which the substitution produces. We must now consider an application of this view to the question of vital action.

On Ehrlich's theory of antitoxins there are two sets of molecules in living tissue: those on the outer surface of the cells being concerned with the nutrition process, while those within are employed in cell metabolism. If the outer, or peripheral, molecules can be put out of action, the cell lacks nutrition and eventually dies. Ehrlich supposes that the peripheral molecules have active side-chains serving to anchor suitable molecules which may be carried past them; and in

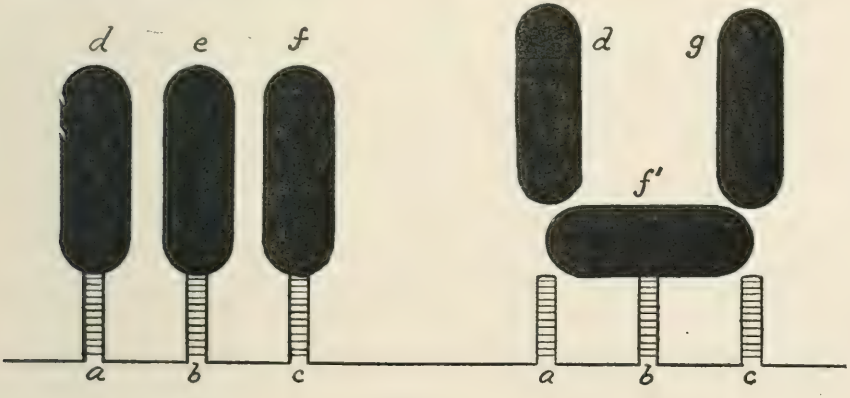
this way the cell extracts the necessary nutriment from bodies carried past it in the blood currents. These side-chains, however, may serve to anchor toxin groups as well as nutritive ones; and, on Ehrlich's view, when a sufficient number of toxin groups have been anchored, the nutrition of the cell may suffer to such an extent that the cell dies.

Schryver[1] has put forward a different view of what takes place. A concrete example will make the matter clearer, and for the sake of simplicity we may symbolise the cell by the benzene tricarboxylic acid shown below:



Here the active side-chains are represented by three carboxyl radicles. Now suppose that a molecule of aniline passes within the range of the central carboxyl group (2), and is seized and anchored by it. The presence of this large group will hinder the approach of other aniline molecules to the carboxyl groups (1) and (3), so that they will be temporarily put out of action until the first aniline radicle is disposed of.

We may represent the action diagrammatically in the following way:

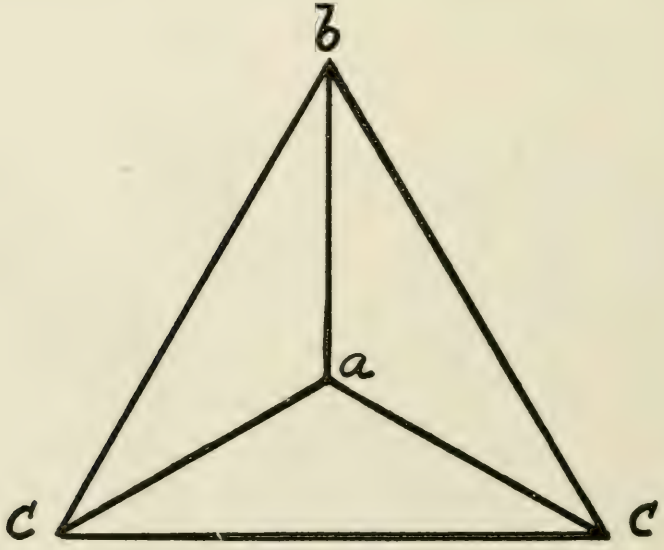


(*a*) FIG. 1. (*b*)

[1] Schryver, *Chemistry of the Albumens*, p. 183 (1906).

In fig. 1*a* the side-chains are represented by the unshaded parts *a*, *b*, and *c*, while the molecules of nutritious substance are represented by *d*, *f*, and *g*. It is obvious that the presence of the molecule *f* on *b* will not interfere to any extent with the action of *a* or *c*. This symbolises the normal action of the side-chains. If, however, the central side-chain *b* should happen to anchor the abnormally large body f^1 (fig. 1 *b*), the state of affairs would be quite different; for f^1 occupies so much room that the side-chains *a* and *c* could not come into play, and the cell would consequently be starved. On Schryver's view, then, toxicity in a tissue may be regarded as due to a steric hindering of normal functions.

Before concluding this article, some mention should be made of the relation between vital action and the production of molecular asymmetry. When in the laboratory we convert a compound $Cabc_2$ into another, *Cabcd*, which contains an asymmetric carbon atom, it might be expected that an optically active product would result. In practice, however, this does not occur; for instead of one antipode being produced, the final product is racemic. A consideration of what takes place in the course of the change from $Cabc_2$ to *Cabcd* will make clear the reason for this. In the substance $Cabc_2$, we are dealing with a tetrahedral arrangement of atoms like that shown below:



The two groups *c* are identical. Now, if the right-hand *c* be replaced by *d* we shall have one compound, while if the left-hand *c* be chosen for replacement we shall have the optical antipode. Since the optical antipodes are identical in chemical properties and differ physically only in respect to their crystalline forms, there is no reason why the dextro form should be formed rather than the lævo form; and there is no reason why one *c* should be replaced rather than the other, since they are symmetrically situated with regard to one another. It is, therefore, only natural that in one half of the total number of molecules the right-hand *c* is replaced by *d*, while in the others the left-hand *c* is chosen, the result being a mixture of the two antipodes in equimolecular proportions.

We may take it as a general rule, then, that from symmetrical substances only symmetrical products are formed in the course of our laboratory reactions. On the other hand, when we examine the constituents of living tissues we find that a very large proportion of them are optically active substances, so that at the first glance there appears to be a profound difference between chemical actions in the laboratory and those which are carried out within living organisms. At present those chemists who take an interest in the matter are divided into two schools. The one school holds that there is no fundamental difference between the chemical processes of vital action and the chemical processes of the laboratory; while the other school maintains that the question of molecular asymmetry sharply divides the two sets of phenomena. Up to a few years ago, neither view could call any experimental evidence to its assistance. The case of sodium ammonium racemate was cited by the "uniform" school as a proof of their case. This substance, if allowed to crystallise from its solution under certain conditions of temperature, separates spontaneously into two sets of crystals which, owing to the difference in the disposition of their hemihedral faces, can be separated by hand from each other. The upholders of the vitalistic theory of molecular asymmetry, however, pointed out that in order to separate the two vital action was necessary, since a human being was a vital agent. It is unnecessary here to enter into the details of the arguments employed on both sides; they are to be found in the correspondence in *Nature*,[1] and were chiefly theoretical.

[1] *Nature*, vols. 58 and 59 (1898-9).

The question appears to have been settled in a somewhat unexpected manner. Cotton,[1] examining the absorption of dextro circularly polarised light by copper ammonium tartrate, found that the two tartrates did not possess identical powers of absorption. Now, if light is absorbed by any molecule, some extremely delicate rearrangement of the molecule's internal structure must take place; for we cannot imagine that a vibration can be stopped without some energy being expended in stopping it or changing its amplitude. Therefore, in the case of the dextro and lævo forms of copper ammonium tartrate, more energy is being used up by the one form than by the other; and hence the intramolecular readjustment is greater in the one isomer than in the other. In other words, dextro circularly polarised light is an agent which favours one form of tartaric acid rather than the other. Now, Byk[2] has pointed out that light is circularly polarised by the surface of the sea, and that one variety of the circularly polarised light predominates on the earth's surface. This obviously furnishes us with an asymmetric agent whose application, as far as the earth is concerned, is nearly universal; and it seems not improbable that some such cause, acting under favourable conditions, may have given rise to a preponderance of a right-handed or left-handed form at one particular period in geological history. Until some active substance is actually synthesised by this method, however, the question must be regarded as, at least technically, an open one.

It should be noted that once the presence of an asymmetric substance is granted, the subsequent steps up to the present complex series of naturally occurring asymmetric forms become simple to understand. A very small quantity of an asymmetric acid, under suitable conditions, may serve to resolve any quantity of a racemic base without losing any of its peculiar asymmetry; while each portion of base resolved into its antipodes becomes a new centre for the resolution of racemic acids, which in their turn may serve to resolve new bases, and so on *ad infinitum*. The case is analogous to that of ordinary animal reproduction, since with each generation the reproductive power increases in geometrical proportion.

In conclusion, it may be pointed out that the accumulation

[1] Cotton, *Ann. chim. phys.* [7], **8**, 373 (1896).
[2] Byk, *Zeit. physikal. Chem.* **49**, 641 (1904); *Ber.* **37**, 4696 (1904).

of further data as to the action of stereo-isomers upon animal tissues is very desirable. Since a slight difference in structure may produce a great difference in the action of drugs, it seems advisable to begin by investigating those cases in which the structures of the bodies are identical, and the only difference lies in the special arrangement of the atoms. At the present time the data in this branch of the subject are only sufficient to show that the influence of stereo-isomerism is of importance; they are not full enough to enable us to draw any general conclusions.

THE PROPAGATION OF PHYSIOLOGICAL CHANGE

A PHYSICAL PROCESS

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It is a leading characteristic of living matter that it is readily excited by slight sudden changes in its environment. To these stimuli it frequently responds by chemical and physical reactions in which much more energy is transformed than was supplied by the exciting cause, in this way resembling all unstable structures and compounds. It differs, however, from these unstable bodies in that it rapidly returns to its original state, abstracting matter and energy from its surroundings to make good its loss—a process which can be repeated indefinitely. Nor is the word indefinitely made use of in any limited sense, as will be apparent when the processes of growth and multiplication are considered. Unstable when momentary changes are dealt with, stable when time is taken in larger units, living matter is usually spoken of as "labile."

Now when an attempt is made to penetrate the meaning of this state there are certain facts which always obtain a primary hearing for circumstances of purely chemical rank. In the first place many of these exciting causes lead obviously to chemical change, as in the case of muscle, from which, when excited, waste products trail away as definitely as the smoke and fumes from any chemical factory. In the second place there is no doubt that all the energy transformed in the body is liberated as a consequence of the oxidation processes by which its food material is overtaken. This statement has within recent years been placed in a position of almost absolute truth.

Explanations of both phases of this condition of lability have therefore most frequently been arranged in terms of chemical reactions. At one time we were invited to think of these vaguely as due to processes of assimilation and dissimilation,

anabolism and katabolism; no very certain limit being placed to the meaning of these terms. Alongside of the processes leading to the formation of an unstable and highly complex product were also processes leading to its destruction, and both sets of processes were characteristic of the normal resting state of living matter. Stimulation led to a new quantitative relation between the two, so that momentarily the destructive process was dominant. During the recuperative period following excitement the constructive process was dominant in its turn.

More recently considerations introduced by Ehrlich, as also the time relations of the oxidation process observed by Hermann, have served to modify and harden this view. It is on the one hand made clear that the destructive process need not be postulated as overtaking more than a certain fraction of the molecule, or, as it is expressed, no more than certain of its side chains. On the other hand also the constructive process can be imagined as repeatedly recommencing from a constantly surviving level of considerable perfection. Verworn has therefore brought to our notice a certain stable atom-complex, or nuclear group, to which more unstable side chains are linked under pressure of ordinary chemical and physical circumstance. In these and between these side chains perpetual change occurs, which is exaggerated and perhaps given a new direction by the incidence of stimulation. Every act of "life" therefore has its basis in the chemical changes taking place on the fringe and within the influence of these residual nuclear molecules. This at least if certain "enzymes" are allowed to play a part less definitely assessed, and take a place in the molecule less definitely assigned.

To satisfy the intimacy of intracellular life the conception has been tentatively extended by a hypothesis first boldly introduced by Pfluger. The residual atom-complexes are linked together to form a giant nuclear molecule which occupies a great share of the space within the cell, as for example the whole of that process of a nerve-cell known as the core of a nerve-fibre. In this way we have in front of us a schema which clearly illustrates the conception that "life" is primarily dependent upon chemical structure, and that all the expressions of life are the outcome of chemical change. This conception has indeed been welcomed as serving usefully to link together many

experimental facts. In the first place those facts upon which it was based, the storage of oxygen within the cell in chemical combination alongside of oxidisable material to which it is finally handed over. In the second place such instances as those in which minute traces of specific chemical materials modify the life and activities of the cell. In this latter case the concept has justified its existence by indicating fruitful lines of experiment. Its influence has also been clearly felt in further elucidating the importance of oxygen not only to life, but also to function.

We must ask, however, whether no position of importance should be accorded to states and changes of a purely physical kind. Surely these also play their part within the cell. Their influence upon extra-cellular material is often paramount, and it is perhaps best by a consideration of this influence that their case may be introduced. Let us for example consider a rod of cartilage. Here cells are situated in a mass of tough material which they have manufactured. Unlike their epithelial cousins situated on the surface of the body, they do not pass their manufactured products on to a surface from which in the course of events these are swept away. The cells are therefore separated by a greater amount of material, which first oozing from them as a viscid mass no more rigid than its source, then sets into the tough elastic substance to which the cartilage owes its physical characteristics and its sole use. Now who can deny the probability that this process of "setting" is due to stimulation provided by the stresses and strains to which the exuded material is at once subjected? We thus get a picture of this extracellular material undergoing a change of physical state of functional importance, and that as the result of excitation. Again, as the cartilage plays a frequently repeated part in the stresses and strains to which the limb is subjected by muscular action and external pressure, certain points within it are selected as the foci through which resultant forces pass. These most probably are the "centres of ossification." The forces in action arrange the cells and matrix in columns radiating from this centre. The stimulated matrix continues with its change of state, the colloid material passes from the condition of a homogeneous jelly into one in which there is a precipitation of denser "calcified" material from a more simple solution readily removed; condensation and liquefaction. Is this a

chemical change resulting from the exudation of a new corrosive substance from the excited cells, or is it a physical consequence of repeated pressure upon the homogeneous jelly? It is clear that the mass primarily stimulated is the jelly, which from its consistence must be held to protect the cells enclosed within it. Again, the irruption of bone-forming cells takes place, made possible by the liquefactive change. Each one of these new cells secretes an exudate which then "sets." The directions taken by the fibres precipitated in the setting bone-matrix at once suggest the notion that they are the result of the stresses and strains to which the material is subjected. Calcium salts also make their appearance passing from a state of solution to be precipitated as granules. In the case, then, of bone-formation it is again the change of physical state which at once claims our interest. Now it may be objected, that there is no proof that these changes are not of chemical rank, perhaps even attended with an output of waste products. However, even if that possibility be admitted, the contrary hypothesis is seen to be at least as admissible, and therefore to require definite examination.

But if a case can be made out for the examination of such possibilities when dealing with extracellular material, the same possibility must also be introduced within the limits of the cell. The difference between similar changes within and without the cell may be no more than a difference of opportunity and advantage. Outside the cell the sole sources of energy are those provided by stimulation and the heat of the circulating solutions of the body. The cell has the advantage here. Inside the cell recovery may thus be more usual than outside of it. Within the cell, as for example in the musculature, is the source of heat production whence the heat of these circulating solutions is derived. Within the muscle-fibre itself this chemical source of energy may play no greater part in many important internal processes than is played by the heat of the forge in the production of steel. If the muscle distributes heat to other tissues of the body which is made use of in this way, what limits are to be set to its own participation in similar advantages? It is a possibility often lost sight of, but none the less important, that the source of energy within the muscle may possibly be responsible for physical states, *for physical stores of potential energy*, the disturbance of which in their turn may give rise

to a development of heat. Heat so produced is to be traced finally to the combustion of food material as surely as that directly produced by the occurrence of concomitant chemical change.

So far I have done no more than consider the possibility that physical state and change may be of importance in intracellular change. Let us consider a case where they must of necessity be introduced—as for example in the secretion of water as sweat at a point upon the surface of the skin. Below this surface there extends a long tubular pocket lined by epithelial cells. These cells are suddenly aroused from a state of rest by the arrival of a nervous impulse dispatched from the central nervous system; after a short delay water wells from the mouth of the pocket on to the skin. Whence does this water come? Certainly not from the side links of any "biogen molecule" within the cell. The water has indubitably been drawn through the cell from the solutions bathing its internal surface. The cell has acted as a pump in which valves give a direction to the work performed. Now how is this to be dealt with if the physical arrangements of the materials forming the cell are not to be placed in the foreground as matters of importance? Discuss these arrangements and it at least becomes possible to conceive a line of experimental approach to the phenomenon.

Let us for a moment suppose that, as is most probable, the internal and external surfaces have characters of a different kind. In some epithelial cells such differences can be seen. In all, even including the nerve-cell, such differences must be inferred. Let us suppose that this cell, set upon the surface of the body, is in the habit of passing some of its waste products, as for example carbonic acid, not back into the solutions of the body, but out on to its external surface. What more probable? But then, if so, we have only to imagine that our exciting nervous impulse quickens up the chemical changes occurring within the cell and so gives rise to the production of carbonic acid. These new molecules, added to the physical value of the cell as a mass of molecules in a state of solution, are the cause of an increased osmotic pressure which is sufficient with the differences postulated between the cell surfaces to account for the passage of water out into the pocket of the little gland. Here we have been forced to think of the matter within the cell as a solution,

of a chemical change as merely adding to the number of molecules in solution, of the cell limits as permitting or restraining the motion of dissolved material. None of the important activities underlying these terms are made obvious by the contemplation of "biogen molecules."

Let us examine an instance a grade more complex. The salivary glands are formed of similar pockets, from which on the arrival of a nervous impulse water again gushes out on to the surface of the body containing in solution organic and inorganic matter. The same explanation will temporarily cover the output of water. But why these other substances also? Now there is no doubt that most of these other materials have lain within the cell long before the arrival of the nervous impulse, having been manufactured during hours of rest from secretion. They are now merely washed out in the rush of water. In addition to the chemical work done on receipt of the impulse we have here to consider specialised chemical work performed quite independently of this, such as is done by gland-cells to which no such impulses come. Is it of advantage to consider the molecules engaged in the one change and in the other as necessarily the same? Is it not simpler to think of molecules of diverse kinds in solution side by side but undergoing a different cycle of change with intensities that reach their maxima at different times? We have been compelled to introduce the concept "solution" to explain the transit of water. There can be no doubt now, as the result of manifold experiment, that this concept has to be introduced in case of every cell; nor even any doubt, to make the matter physically more complex, that the variations of phase within these solutions undergoing change with every kind of alterations in physical circumstance have all to be taken into consideration as modifying the activities of the cell, and therefore as variable physical states of functional importance.

Now it may be said that, after all, these instances have done no more than attribute important results to chemical change as the primary source of energy directly involved in the processes considered. The carbonic acid production has been dealt with as the source of the change. But what if we select as our new molecule adding to osmotic pressure not carbonic acid, so obviously the outcome of an oxidation process, but some inorganic salt? In the case of the salivary secretion certain

experimental facts render a consideration of this possibility necessary. Here the chemical side is not so obvious, since we may suppose these molecules to have been pre-existent within the cell, and to be liberated not from a position of chemical combination, but from some position of physical restraint on arrival of the nervous impulse. This is a possibility with an important distinction. The whole sudden energising of the cell is made to take an entirely physical form. In this case, however, the chemical side must not be lost sight of. We must suppose that, during its rest from secretion, the chemical energy of the cells was utilised in producing this physical condition of restraint, and so feeding a secondary source of energy conveniently available at the moment of stimulation.

And now, having prepared the way by means of allegory, I would turn to the specific instance which I am more concerned in protecting from this purely chemical conception of function. If living material is at one time stable and unstable, is labile, where is this fact more obvious than in the case of nerve? Excited by almost every slight change of environment to a display of function and therefore most unstable, but recovering so rapidly that within one five-hundredth of a second it can be provoked to an exactly similar change of state—a process which under favourable conditions can be repeated from dawn to dewy eve without giving rise to any trace of fatigue. Pancreas cells may be unexcited to their specific function save on the addition of certain specific chemical materials. Mammary-gland cells may preserve a similar scorn for fleeting change. In them obvious chemical work is done, and the chemical composition of the media in which they lie is their main concern, of which they are not oblivious; but of their extreme lability what proof have we? Astonishing pictures of exhausted cells indeed form evidence for an opposition. Turn to nerve-fibres and the facts are curiously reversed. Most stable and yet most unstable, "labilissimi," what proof have we of the primary importance of their chemical constitution and its changes? Practically none. I shall escape impeachment for this statement. The main fact is so clear notwithstanding some slight evidence to the contrary. Thus Waller has in most ingenious fashion shown that prolonged stimulation leads to just such a modification of conduction as is producible by carbonic acid, but not unproducible

by other factors also. Bethe has shown that prolonged stimulation is followed by an alteration in staining capacity with certain basic dyes, a circumstance which may have no more meaning than an alteration in physical state. It is true that this absence of chemical evidence may be ascribed to the minute quantity of change involved, and to the swift flight of the nervous impulse.

It is no doubt true that the maintenance of life in the nerve-fibre is dependent upon chemical factors. As is the case with all intracellular material, the continuance of its function is ultimately dependent upon a complex of chemical and physical states to which the cell-nucleus, nutriment, oxygen, and the removal of waste products such as carbonic acid are essential. Doubtless, too, this complex is affected by narcotics in some cases, in a manner to be assigned to the inhibition of certain essential chemical processes. No doubt also drastic interference with this complex may produce an abnormal physico-chemical state in which symptoms of fatigue become obvious, as in the instance of the effects of aconitine observed by Waller, chloroform by Garten, lack of oxygen by Frohlich. But here we have our fingers upon the very acme of lability. This is the tissue from which many of the conceptions of lability of living matter are derived. It is significant that here of all places we should have to hunt so desperately for instances of chemical change. There is, then, excellent reason why, on a basis of experimental fact, nerve should not be used in illustration of the value of any purely chemical concept of function.

Thus that rapid propagation of physiological change from point to point which is the leading function of nerve, but is by no means confined to nerve, is doubtless always due to the conveyance of a physical change. The train of gunpowder is apparently a simile to the contrary, but I believe I am right in insisting that in this case the explosion of each segment in turn is due to a rise in temperature. That physical factors are alone responsible for the similar excitation of each successive piece of nerve has been laboriously proved by a study of the action of every admissible form of stimulus. Efficient physical forms of stimulation are, the incidence of light, a rise of temperature, a slight mechanical shock, pressure, a sudden fall of electrical potential, sometimes a fall of temperature, sometimes a rise of electrical potential, sometimes the removal of

pressure. Every sudden change in physical environment is liable to stimulate nerve. Efficient chemical means are all illusory. Only such chemical reagents as have a capacity for indirectly producing physical change are capable of stimulating. Their effects are then in proportion to the physical change which they are capable of producing, as has been well shown by Matthews. Solutions of electrolytes produce electrical effects. Solutions of varied concentration cause variations in the water content of the nerve. Concentrated solutions by concentrating the electrolytes in the interior of the nerve give rise to self-stimulation. The transference of the excited state from point to point of the nerve can therefore be assigned with certainty to physical change, and this no matter whether the disturbance at each point thus affected involves chemical change, such as the decomposition of a carbohydrate or a fatty side chain, or physico-chemical change such as the disruption of some adsorption complex, or purely physical change such as the conferring of new motion upon matter in solution. There is, in fact, no experimental datum which would enable us to think of any other factor than physical change as responsible for the transmission of the excited state from point to point. We are, therefore, debarred from accepting in this case many of the mechanisms which have been suggested, such as, for example, the transference of an oxygen atom from side chain to side chain of an excited giant molecule.

Compelled thus to retreat to physical change alone, there can be little doubt as to the nature of the physical change which is responsible. Most careful experimental inquiry has failed to obtain any evidence of heat-production such as occurs in the train of gunpowder. As to the occurrence of an electrical change of very considerable magnitude, such as has been most beautifully measured by Gotch and Burch, there is no question. This in fact is the only kind of change which has been found to accompany the transit of a nervous impulse. The point, therefore, which has seemed long to Hermann and others of capital importance should be stated boldly. *No matter what may be the complete change at each excited point, its transference from point to point is accomplished by electrical agency.* To this statement the absence of any temperature change enables us to make an important addition. This excited state which is propagated by electrical change is certainly not a chemical

process such as is attended with a consumption of oxygen, nor with the transit of oxygen from less stable to more stable combination. And this is equivalent to saying that, whereas oxygen may play a considerable part in the maintenance of the normal state of nerve, it plays no part in its function. We are obliged, therefore, to consider the case of nerve apart from the case of muscle, where each functional display is attended with a production of heat and a discharge of carbonic acid. But is this statement as true as it at first sight appears? In muscle function there are two clearly separable phenomena, the propagation of an electrical change and the consequent contraction. The propagation of electrical change is undoubtedly a process of the same rank as the process studied in nerve. With regard to this moiety of muscle function wherein muscle resembles nerve, there is likewise no evidence of any production of heat or output of waste product. The electrical change is over before the occurrence of that functional display which is usually thought to be associated with these factors. In the case of muscle, when acting as nerve, the facts are obscured by a later exhibition of another function. In the case of nerve itself the facts are clear. There is nothing but an electrical change observable.

It is not marvellous in view of such evidence that investigators have been found bold enough to plan a line of experimental attack upon the mysteries of nerve function, based primarily upon the assumption that the materials present in nerve are important solely as conductors, insulators, dielectrics. This is a very different assumption from that underlying the "biogen molecule," and yet in this case it has proved of the utmost value. If the purely physical concept of nerve function is finally set on one side, it will be set on one side as the result of evidence collected upon these lines. Some of this evidence has favoured such a concept, some of it has served materially to limit its range. The peculiarities of the electrical conductivity of nerve have been closely studied, sometimes deliberately, sometimes as a series of data which would one day acquire biological interest in connection with the characters of protoplasm. Both sets of workers have used the same methods. An electrical current is passed through a nerve and data are collected from the "polarised" region as also from the extrapolar stretches. For instance, in the anodal extrapolar region

each point nearer to the pole acts as if at a higher potential to points more remote; in the kathodal extrapolar region near points act as if at a lower potential than points more remote.

Now, when an attempt is made to explain these facts, there is no doubt but that we must first consider all the peculiarities of the complex arrangement of electrical conductors represented by the nerve-trunk. Until that is done few will listen to the appeal: let us be simple, we are studying life. Physical models built of materials of different conductivity have been constructed to illustrate the peculiarities found. These models form a physical scheme for a simplified conception of the arrangements of conductors within the nerve-trunk. The inquiry is then pushed further. Can electrical impulses be propagated along such models? Certainly. Well then, are we to consider the nervous impulse as a phenomenon of a type akin to these? Boruttau, assiduous worker upon the electrical phenomena of nerve and upon such illustrative models—that is to say, an examiner of both sides of this question—answers in the affirmative.

Now, before deciding the extent of the reliance we will place upon this answer, we must note an important fact. Nervous impulses, whatever their essential nature, are propagated by electrical agency. They are also propagated along contiguous fibres in different directions without in any way modifying one another. Nerve-fibres therefore represent isolated conduction, and, as we have said, this entails electrical insulation. That this insulation is by no means perfect follows from the fact that electrical currents can be made to traverse nerve-fibres, and also that the electrical phenomena occurring within the fibre are demonstrable upon its surface. It might easily be shown, however, that this imperfection as revealed in both of these facts is of such a kind as to confirm the main statement when put in a slightly different form. The sheaths of nerve-fibres profoundly modify the motion of electrolytes. It follows therefore, no matter what may be the nature of the excited state of nerve, that the study of the electrical conductivity of nerve must be expected to develop information such as will be gained from the examination of any set of imperfectly insulated conductors. The core-model can be foreseen, and its invention cannot be at once accepted as revealing essential facts. It is not enough to have become acquainted with the insulation of the

central conductor. Before committing ourselves to any dogma it is necessary to have studied the character of the core. We must, in fact, find data upon which to consider the possibility that the nervous impulse is no isolated phenomenon, but one that has its analogue in the conveyance of excitation from point to point within all kinds of cell structures where insulation is neither discoverable nor theoretically required. Boruttau has appreciated this necessity. He has transferred the facts of the core-model to the central core. But for this transference he has, in my opinion, no warrant. If the core of the individual nerve-fibre does really possess a structural arrangement similar to that of the core-model composed of its several parts, that fact has yet to be demonstrated. The facts elicited so far are of a different order. Boruttau has, so to speak, abandoned a consideration of the sheath without omitting the very important facts for which this structure is responsible from the experimental data made use of to present the case in this new sheathless form. On this, and on other grounds also, we for the time being fight shy of this core-model theory of nerve function.

Do considerations such as this, then, drive us back into the arms of the biogen molecules? Certainly not at present, since the needful factors seem at least in great part physical. The core has still to be studied as a conductor of electricity, that is to say, as a solution of electrolytes. What are the electrolytes of nerve? How are they distributed within the nerve-fibre? How is the motion of electrolytes which takes place within the core during the conduction of a nervous impulse to be explained?

Now if at first sight it seems absurd to treat of this core, this strip of protoplasm which is responsible for so much, as a solution of electrolytes, let us remember our treatment of the case of the cells in the sweat-gland and in the salivary gland. In putting nerve material in such a category we do nothing new or unexpected. This criterion "solution" is one to which every cell has to be compared, and from it much information illuminating the basis of function has already been obtained. The general considerations which entail this treatment are present in nerve as elsewhere. Thus the core exerts an osmotic pressure upon the surfaces inclosing it, and this pressure is normally balanced by the pressure of the solutions outside. Alter the character of these solutions ever so little and this is at once seen.

Immerse a nerve-trunk in water or in dilute solutions, it becomes turgid, and in consequence as rigid as a stick. Immerse it in concentrated solutions, it loses in weight and becomes abnormally crenated and limp. Watched under the microscope individual fibres may be seen to undergo corresponding change. Passing in review the materials in solution within the core it is at once obvious that, as in the case of other tissues also, the number of molecules of simple bodies like the inorganic salts is overwhelmingly great in comparison to the number of the huge protein molecules with which the concept "biogen" is inextricably associated. Experiments of this type are of such a nature as to give us some knowledge of the number of molecules within the core, a number to which the inorganic salts present contribute a share so great that the contribution of other matter is negligible.[1] From such experiments there is promise therefore of obtaining a quantitative value for the inorganic salts, and that is almost equivalent to the value of the electrolytes within the core. Here, then, is a road to the knowledge immediately required to fill the gap in the data requisite for a proper discussion of the core-model theory. It is unfortunately the case, however, that this road has been shown not to be a high road. The information obtained is not of such direct value as it seems, since it has been found that a balance is not necessarily obtained by equalising the salt-content of the internal and external solutions. Such knowledge, then, is of value, but is apprised as only one of the items necessary to a complete understanding of the case.

We expectantly turn, therefore, to another method which promises to provide further information as to this core in so far as it can be treated as a solution of electrolytes. An electrode placed upon the core exposed at a cross-section of a nerve-trunk is electrically negative to a similar electrode placed upon the uninjured longitudinal surface. This fact is with great probability to be assigned to a difference in the solutions present in these two situations, and to the nature of the membrane formed by the sheath of the fibres, which limits the diffusion capable of taking place between the solutions in the interior of the nerve-trunk.

In the concentration cell so formed one of the solutions

[1] Carbonic acid molecules are not considered in this case, since the surfaces are permeable to them.

is accessible, its character well known; this is the external solution or lymph, which surrounds the fibres and bathes their longitudinal surface. Modifications in this external solution produce modifications in the electromotive force of the concentration cell. Such modifications have been carefully studied, and it has been shown that they are quantitatively consistent with this assumption made as to the nature of the source of E.M.F. The external solution being varied, the internal solution is found to behave as a solution of electrolytes of such constancy that the data obtained from one nerve by one variation of the external solution are capable of accurate contrast with the data obtained from another nerve with another variation. The facts are so definite, indeed, that it is possible in this way to determine the concentration of a solution, by placing it upon the surface of a nerve instead of the normal external solution and observing the alteration produced in the E.M.F. of this concentration-cell. Here, then, is another road along which to obtain a knowledge of the internal solution of electrolytes. Given one condition it can obviously be made a high road to their quantitative determination. If the specific nature of these electrolytes were known, then that strength of their solution which would reduce this E.M.F. to zero would coincide with the strength of the internal solution. In the experimental work done in this subject this point has been realised. There is evidence, recently strongly reinforced by the use of microchemical methods introduced by Macallum, that the electrolyte concerned is potassium chloride. Doubtless this point, its importance once realised, will be settled by such direct determinations of the salts in nerve as is now being undertaken by Alcock. It is significant of the times and of the ideas which have been introduced in competition with such concepts as "biogen molecules," that so much effort is being expended upon the decision of this question. Assuming this internal electrolyte to be potassium chloride, then it has been shown that the solution present within the nerve-fibre is of such an extraordinary concentration that it is impossible to think of such a concentration as normally present there. Here then we have a handsome paradox, but one for which there is possibly a clear explanation.

When nerve-fibres are injured as by a cross-section, they are excited. If it is assumed that the excitation is a fatal one,

from which the fibres do not recover, there is then present at such cross-sections a stationary excitation. Now we have shown that the transference of excitation is a matter definitely to be set on one side as due to an electrical agency—that is to say to a motion of electrolytes. Since we must think of this motion of electrolytes as produced by an alteration of the character or quantity of electrolytes present at an excited point, we may expect to find at a fatally excited point the new or additional electrolytes thus known to be characteristic of this state.

There is no evidence that the nature of the electrolytes is new. Here we are down upon an old bone of contention. It has been suspected that a decomposition process occurred at such injured points attended with an output of carbonic acid and waste products of a similar kind. There is, however, no acidity to be detected. If there were, the amount necessary to account for the phenomenon would have to rise to a value instantly detectable. This is shown by the fact that this injury current, which may be modified by alkali as by any other electrolyte, is not annihilated by a value of caustic soda solution below 0·8 per cent., and that solutions of hydrochloric acid may equally well be used for this annihilation. Place this beside the fact that it is possible to identify the additional potassium evoked by the injury by microchemical means, and it seems certain that injury produces not an electrolyte of a new character but an additional quantity of such electrolytes as were already present. A study of the injury current therefore may be taken to have revealed the existence of a solution of extraordinary concentration in the excited region of the nerve-fibre. It seems obvious that excitation may modify the strength of the solution present in one of two ways—either by withdrawal of water for combination with some more solid material not to be reckoned as part of the "solution," *e.g.* in a solid phase, or by release of potassium salt from under cover of some such solid phase into the quantity of material to which the general character of "aqueous solution" is due, or from some state of chemical combination.

The terms in which the last sentence are couched are the result of a microscopical study of the core of the nerve-fibre. It is impossible long to pursue a study of nerve-fibre structure without being impressed by the colloidal character of the

solution within the core. On immersing fibres in solutions of different concentrations attention is at once drawn to the precipitations and re-entrance into a state of solution of the colloid material present. Such variations have also been observed by Höber in nerve-fibres treated with solutions of electrolytes, and have been inferred as of probable occurrence by Matthews, Greeley, Loeb, Lillie, and others. Nor is there any need to vary the character or concentration of the surrounding solution to observe these effects, since a distribution of varied states of colloidal solution is discoverable in nerve-fibres immersed in the solution usually first made use of, such as "normal saline" or "Ringer," solutions somewhere about the concentration of a decinormal solution. The states thus observed have a definite relation to injury and to the injury current which arises from it. At the cut end, the source of this current, there is some precipitation of colloid matter from solution; this portion is limited by a stretch of limpid solution, and this is again succeeded as we reach kathodal points, where the current is leaving the nerve-fibre to enter the surrounding solutions, by successive tracts showing greater and greater degrees of precipitation.

But this is an observation pregnant with suggestion, since the conditions present at "kathodal points" are as of great an interest as those present at the injured region. If there were hesitancy in postulating something of the nature of excitation as influencing the conditions due to injury, there can be none in case of the kathode. The institution of the kathodal condition has been shown to be the cause of that excitation which follows the admission of an electrical current into a nerve. During the closure of the current the whole region of the kathode is in a state of increased excitability. It is significant, therefore, that at such points there is again a visible alteration in the core. The importance of this fact is enhanced by microchemical proof that in this district also there is present a solution of a potassium salt of much greater concentration than that to be found in any other part of the nerve-fibre except the region of injury. There is therefore some basis for the statement, that *at every excited point there is a coarser aggregation of colloid material, accompanied by the appearance of a new quantity of potassium salt in a state of simple aqueous solution.*

Apparently, therefore, the facts discoverable under these

headings of "injury" and "kathodal state," and the circumstances inferred from this as characteristic of excitation, are of the same order as those observed as a consequence of the excitation of extracellular material—as, for example, in the case of bone matrix, in which the setting of the matrix was attended with a demonstration of calcium salts.

It is necessary to explain at this stage an omission, which to the expert may seem of much account, namely, the absence of any reference to the finer details of the minute structure of the core. Histologists, whose views of structure are too frequently obtained from tissues altered by the precipitating effects of "fixatives," describe in this core long lines of a more refractive material which run parallel one with another and are separated by and surrounded with material of a simpler kind. The critic may say that the considerations which have taken us within the core should have taken us further. The point assumes importance since from several sides these neurofibrils have been accredited with the functions of the fibre. I have myself made theoretical use of such structures, but have been compelled to abandon them in consequence of a prolonged acquaintance with the appearance of nerve-fibres immersed in the fresh state in simple saline solutions. Neurofibrils are not seen under such conditions. This being the case, their admission into the summary of experimental evidence is hardly justified. It will be remembered that in the case of bone-formation fibrils are precipitated in the material as it sets, and that their arrangement is possibly assignable to forces in action at the moment of setting. It is possible that neurofibrils are similarly formed. If so, it is also possible that they are formed at a time when the setting of the colloidal solution of the nerve-fibre has attained a degree not arrived at during normal functional life.

Here at last, at the end of the evidence, one feels the demand for a constructive policy to guide further experimental work. If biogen molecules and "ion proteids"—the latter representing a concession, it is true—obscure many points of physical interest in the structure and function of nerve-fibres, and if the core-model theory is set on one side whilst a debate is arranged as to the nature and meaning of neurofibrils, what conception can be placed in their stead? Is there any "opening" along which the game can be further played with as much success as along

the openings thus discredited? This is a claim which I have ventured to satisfy.

Let the strip of protoplasm forming the core of the nerve-fibre be thought of, not as a homogeneous mass of similar molecules, but as a *solution* in which co-exist molecules of very different sizes and kinds. For simplicity let us fix our attention upon two kinds of molecules exhibiting these differences in their utmost extreme—one kind large colloid molecules tending to form aggregates, the other kind minute inorganic salt molecules and ions mutually avoiding one another but tending to be drawn into association with colloid aggregates. Imagine also that a certain average size of aggregate represents the normal condition of colloid solution in the nerve-fibre, but that departures from this average occur as a consequence of physical change or stimulation. The observation of precipitates at excited points justifies the hypothesis that an increase in this average size is a characteristic of excitation. Now let us suppose that these aggregates or solid phases, or—less correctly, but offering a readier picture—solid particles, attract the salt molecules to their surfaces, so that layers of a condensed salt solution are found thereon. It follows that, when excitation takes place and the number of solid particles is diminished, they in this new condition must offer a smaller total surface for the retention of such a condensed solution of inorganic salts. As a consequence a new mass of inorganic salt will have found its way into the general bulk of aqueous solution surrounding the colloid masses. We thus obtain a picture of the excited state in a form which satisfies all the experimental evidence, and the picture at the same time lends itself to a clear representation of the mode of propagation of the excited state—*i.e.* nerve impulse. To ensure the propagation of such a state in the manner observed in nerve, all that is necessary is to postulate that the exciting negative anion of the inorganic salt present should have a greater velocity than the positive kation. The salt is unmasked, it diffuses, the next segment is rendered negative, desolution accompanied by the unmasking of a further quantity of salt in this new segment, further diffusion, etc., etc.

Now if I am asked at what rate this change would be propagated—and this is a question of great importance—unprovided myself with more than vague uncertain murmurings I deliberately steal another's thunder. W. Sutherland

has conceived a physico-chemical explanation of the nature of the state of colloid solution, and of the nature of the nervous impulse. The molecules capable of entering the state of colloid solution are possessed of pairs of unsatisfied valencies carrying equal quantities of electrical charge of opposite sign. By reason of this quality these molecules can unite with one another to form giant molecules in which the original units are seen as "semplars" joined end to end or side to side, forming a long frame, around which and in the interstices of which the solvent lies. These giant molecules, and the core of the nerve-fibre is one such, being elastic respond to a shearing strain by the transmission of motion along the mass of the molecule. This transmitted state represents the nervous impulse, and at first apparently by a purely mechanical conception. But in the propagation of this strain opposite electrical charges are supposed in each segment of the molecule to be moved one upon the other. Upon such a basis Sutherland has worked out the probable rate of transmission of such a process, and finds it of the same order as the transmission rate of the nervous impulse. To me it seems that the mathematical treatment of this hypothesis involves considerations of exactly the same rank as those involved in the hypothesis I have advanced. The alteration supposed to occur in the colloid aggregates compares with the molar charges assumed to occur in the framework of the giant molecule, and there is the exactly analogous displacement of electrical charges, and in both cases that is all.

Should Sutherland's conception of the nature of the nervous impulse, or should mine, appear too bald or too far removed from considerations of biological rank, I would suggest that this function of transmitting change is of all the functions of living matter the most widespread and the most simple. It is probable that a similar change is transmissible within the limits of every cell in the body. This fact—and I would make bold to say that it is a fact—is frequently lost sight of in admiration of the complex results obtained as a consequence of the transmission of these changes into districts where more complex happenings occur. The conduction of nervous impulses is sometimes discussed side by side with the precise value of states of consciousness, although there is no evidence that the actual process by which change is propagated has altered in the course of development from the protozoon to man.

THE YEAR'S VERTEBRATE PALÆONTOLOGY

BY R. LYDEKKER, F.R.S.

The following article is written on very similar lines to the one dealing with the same subject in this journal a year ago; and the introductory remarks in the latter will in the main apply in the present instance. It may, however, be well to repeat the statement that the publications referred to do not (and from the nature of the case cannot) represent anything like a complete record of the year's work in the palæontology of vertebrates. They are, in fact, merely the more important of those which have come under the writer's personal notice up to the date of sending the proof of the article to press. The opportunity has been taken to refer to a few papers published in 1906 which had not come to hand when the article for that year was compiled.

As was the case last year, there appears to be no startling or epoch-making discovery in this branch of palæontology to record; although a large amount of valuable work, either in describing new forms, or (which is at least equally important) adding to our knowledge of those already known to science, has been accomplished. From the standpoint of geographical distribution, the most important event of the year in the present department of science is undoubtedly the discovery of remains akin to the golden moles of Southern Africa in the Tertiary rocks of North America; a discovery which largely aids in explaining the anomalous distribution of this and the kindred families of the Insectivora.

Commencing with papers dealing with stratigraphical and faunistic palæontology, reference may first of all be made to one by Prof. H. Osborn, published in the *Bulletin* of the American Museum of Natural History, vol. xxiii. p. 237, on the Tertiary mammalian horizons of North America. Although confessedly a preliminary introduction to a fuller work, this paper gives much valuable information with regard to the

correlation and geological ages of the various beds; a noticeable feature being the introduction of diagrammatic sketches of typical local sections in the Rocky Mountain basin and the Bad Lands of Nebraska and Dakota, by means of which the equivalence of the various strata can be seen at a glance.

Closely connected with the foregoing is an article in the May number of *The American Journal of Science*, by Mr. F. B. Loomis, on the probable mode of origin of the Wasatch Eocene deposits. The relative scarcity of fish-remains forbids, in the author's opinion, the view that these beds were deposited in lake-basins. Nevertheless the occurrence of remains of crocodiles, freshwater terrapins, and fishes in sufficient numbers to make up 10 per cent. of the whole fauna, would be very remarkable in strata of æolian origin; and the presence during deposition of a certain amount of water seems therefore to be essential. It is accordingly suggested that the Wasatch beds were laid down on plains subject to periodical inundations; confirmation of this being afforded by their lithological constituents.

Californian cavern exploration has been continued by Mr. E. L. Furlong, who in a note to the March number of *Science* communicates the results of a preliminary examination of a cave near Auburn. The remains found included those of the ground-sloth *Megalonyx*, as well as of pumas and horses; but no traces were discovered of the extinct ruminants *Eucerathcrium* and *Preptoceras* characteristic of the Shasta caves explored by the same gentleman a few years ago.

In a paper published in the *Annals* of the Carnegie Museum, vol. iv. No. 1, 1906, Mr. O. A. Peterson describes a number of new mammals from the Miocene of Nebraska and Wyoming. Several of these belong to the group of oreodonts or merycoidodonts (the "ruminating hogs" of Leidy), but there is also a primitive camel, as well as a dog-like carnivore of the genus *Amphicyon*. The Miocene fauna of South Dakota has occupied the attention of Dr. W. D. Matthew, who has published the results of his investigations in the *Bulletin* of the American Museum of Natural History. The contents of this paper are noticed later.

In Europe Dr. Max Schlosser, in the *Neues Jahrbuch* (1907, vol. ii. p. 1), has given descriptions of fossil mammals from the Pliocene of Spain, with observations on the line of division

between that epoch and the Miocene. A similar service has been accomplished by Mr. F. Roman (*Comm. Geol. Portugal*, 1907) in regard to the mammalian fauna of the later Tertiary strata on the right bank of the lower part of the Tagus valley. In addition to remains of *Mastodon*, *Hipparion*, etc., the author has described those of an apparently new species of rhinoceros. Notices of later Tertiary mammals from Hungary will be found in a paper by Mr. W. Güll, and others, published in the *Mitteilungen Ungar. geol. Anstalt*, vol. xiv. p. 283. Similar brief mention will suffice for a paper by Mr. A. Zdarsky on the Tertiary mammal fauna of Eibiswald, published in the *Jahrbuch* of the Austrian Geological Survey (vol. lvii. p. 437).

A work of an altogether unique type is Mr. Charles Depéret's *Les Transformations du Monde Animal* (Paris, 1907). Its main object is to discuss our present knowledge of the laws regulating the modifications of animals, from the date of the first appearance of life on the globe to the present day. After a preliminary review of the rise of modern ideas with regard to evolution, the author discusses the variability of species in space and time, the constituent elements of phylogenetic "trees," the causes of the extinction of species, the formation of new types, and, finally, the important *rôle* played by migration in the evolution of faunas.

During the year the vertebrate fossils of the Fayûm district of Egypt have attracted American collectors; and to the October number of *The Century Magazine* Prof. H. F. Osborn contributes an illustrated article describing his experiences while in search of such remains, with remarks on the nature and origin of the fauna and their bearing on the geographical distribution of mammals. Among the illustrations, attention may be directed to restorations of *Arsinöitherium*, *Hyænodon*, and *Zeuglodon*. In Eocene times the Fayûm, in the author's opinion, was a savanna-country, partly open, partly covered with scrub, and partly with forest; the temperature being much the same as at the present day. That Africa (then much less extended to the north than at the present day) was the home of the ancestral proboscideans, sea-cows, hyraxes, and probably hyænodonts, Prof. Osborn is convinced; but the absence in the Eocene deposits of remains of primitive hippopotamuses, ruminants, horses, and rhinoceroses, leads to the further inference that "none of these quadrupeds had as yet reached

Africa; that they were evolving elsewhere, either in Europe, Asia, or North America, and preparing for the great interchange of life which would occur when Africa should again be connected with the other continents."

The explorations carried on in the Fayûm by Dr. C. W. Andrews resulted in the acquisition of a few specimens of interest, one of which (as mentioned in *The Geological Magazine* for March) served to demonstrate that the supposed genus *Phiomia* is really based on a portion of the lower jaw of a young individual of a species of the proboscidean genus *Palæomastodon* retaining the milk-teeth. Very curious is the discovery of the existence of a huge globe-like chamber on the side of the lower jaw of *Geniohyus*, which in life may have contained a gland. There are certain indications that this genus, which was originally regarded as one of the Suidæ, may be related to the hyraxes—an interpretation which would accord best with Prof. Osborn's view. The existence in the Fayum Eocene of remains of a large bird related to the herons is a discovery of considerable interest.

Egypt is, however, by no means the only part of Africa from which discoveries of fossil vertebrates have been recorded during the period under review; and from the fact that mammalian remains have been described from three distinct localities at the opposite extremity of that continent, we may expect in the future to obtain further important information with regard to the past history of the South African fauna. The first of the papers referred to is published in the final report of a recent geological survey of Natal and Zululand, in which Dr. W. B. Scott, of Princeton, New Jersey, gives a description of the two last lower molars of an extinct elephant obtained, in company with remains of rhinoceros, hippopotamus, and antelopes, from a deposit of late Tertiary age in Zululand. The elephant, for which the author proposes the name *Elephas zulu*, has the constituent plates of its molar teeth more numerous and thinner than those of the existing African species. They are described as being to a great extent intermediate in this respect between the latter and those of the extinct European and Asiatic *E. antiquus*; but according to the figures they seem much nearer the molars of the species last named. It is suggested that *E. zulu* may have been the ancestor of the living *E. africanus*, in which case it would almost be imperative to regard the molars of the

latter as being of a degenerate type. This question requires careful consideration; but, apart from this, the discovery is one of great interest.

The diamond-diggings at Kimberley are the place of origin of the second batch of remains, which have been described by Dr. E. Fraas of Stuttgart in the *Zeits. deutsch. Geol. Ges.* for 1907 (pp. 232–43). The specimens include an imperfect mastodon tooth, probably belonging to the same species as the one described last year from Barkley West; and likewise other teeth referable to a zebra, a hartebeest or blesbok, and a hippopotamus. In most instances no attempt is made at specific determination, but the author refers the hippopotamus remains to a large race of the common African species.

The third discovery, reported by Messrs. F. Mendell and E. C. Chubb in *The Geological Magazine* (pp. 443–8), relates to Rhodesia, where remains of existing African mammals have been found associated with stone implements of a palæolithic type.

Passing on to a survey of papers devoted more or less exclusively to particular groups of mammals, brief reference must suffice to one by Dr. F. Ameghino of Buenos Aires in which the author claims man to be of South American origin, his ultimate ancestor being the Tertiary opossum-like genus *Microbiotherium*. Such a phylogeny is not likely to be accepted by naturalists. This paper was published at La Plata.

In March Mr. H. F. Standing read a paper before the Zoological Society of London on extinct Malagasy lemurs, in which he discussed their relations to living lemurs and the higher Primates. The remains were obtained in the muddy bed of a swamp formed by the blocking-up of the River Mazy by a lava-flow, at from a few inches to three or four feet below the surface. They consisted of a large number of skulls and limb-bones of lemurs and lemur-like animals. This material enabled the author to corroborate the view, previously put forward by Dr. Forsyth Major, that the extinct lemurs of Madagascar were, in many respects, intermediate between existing lemurs and monkeys, and to express his belief that New World monkeys and the Lemuridæ, as well as the Malagasy Indrisinæ, had a common origin. He also stated his opinion that, in view of recent additions to our knowledge of lemuroids, and from the evidence of the new collection as to their close relationship to

apes, it was no longer possible to separate the Primates into the suborders Lemuroidea and Anthropoidea. The full text of this communication was not published at the time when the present article was written.

Practically nothing has been accomplished during the year to add to our scant knowledge of the past history of bats. It is true that Mr. G. E. Mason, in the September issue of *The Annals and Magazine of Natural History*, described the remains of a supposed new fruit-bat from Round Island, near Mauritius. These remains occur in a guano-deposit, and appear to be very recent; and in a note on Mr. Mason's communication published in the October number of the same journal, Dr. Knud Andersen pointed out that they appear to be inseparable from the existing *Pteropus rodricensis*.

On the other hand, some very important evidence has been obtained with regard to the palæontology and past distribution of one group of Insectivora. The golden moles (Chrysochloridæ) form at the present day a group entirely confined to Southern and South-Eastern Africa, whose nearest relatives are apparently the tenrecs of Madagascar. An extinct Patagonian insectivorous mammal (*Necrolestes*) has, however, been regarded as a member of the same group; and, more recently, Dr. W. D. Matthew (*Science*, December 14, 1906, and *Bull. Amer. Mus. Nat. Hist.*, vol. xxiii. art. 9, 1907) has announced the discovery in the Miocene strata of South Dakota of the remains of what he regards as an undoubted golden mole, although referable to an extinct genus (*Arctoryctes*). The author also considers that the North American Tertiary genus *Xenotherium*, previously regarded provisionally as a monotreme, is a member of the same group. If the identifications be trustworthy, the discovery is of much interest from the standpoint of geographical distribution. Although its bearings cannot be fully discussed here, it refutes the idea that the Patagonian *Necrolestes* affords any evidence in favour of a former Antarctic land-connection between Africa and South America.

In the paper on the Lower Miocene fauna of South Dakota referred to above, Dr. Matthew, in addition to describing certain new species, discusses the relationship of the Tertiary Canidæ to their existing representatives. In his opinion the Old World *Cyon* and probably the South American *Speothos* (*Icticyon*) are derived from *Temnocyon* of the John Day Tertiary; the

characters of the skull and skeleton confirming the evidence afforded by the dentition. The more typical South American Canidæ appear, on the other hand, to be the nearest living representatives of the middle Tertiary *Nothocyon* and *Cynodesmus*; *Mesocyon* being slightly off the line in the direction of *Temnocyon*.

The generally accepted classification of fossil Canidæ is, moreover, in the same author's opinion, misleading; more especially the association of the genera *Cephalogale*, *Simocyon*, *Oligobunis*, *Enhydrocyon*, and *Hydrocyon* in the single subfamily Simocyoninæ. The two last-named genera are, for instance, probably not separable, and together with *Philotrox* appear to be related to *Temnocyon*, and so with the modern *Cyon*; while *Oligobunis* is probably referable to the Mustelidæ. Again, *Cephalogale*, which has been regarded as ancestral to the bears, seems rather, judging from dental characters, to be related to the typical members of the family (Caninæ); such aberrant features as it possesses in this respect being to some extent paralleled in *Cynodesmus*.

Here it may be well to quote a very important note on classification generally appended by Dr. Matthew to his observations on extinct dogs. It appears, he writes, "obviously improper to separate two closely allied genera or species because they are destined to give rise in the future to two distinct families, in the opinion of the investigator. So far as classification and nomenclature are based upon the facts of structural resemblance or difference, they are permanent, but the attempt to make families, genera, or species correspond with hypothetical phyla is not only misleading but destructive of any approach to uniformity or permanency of arrangement."

These remarks the present writer may take the opportunity of endorsing. An attempt has been made to introduce such a phylogenetic classification in the case of the early Perissodactyla, and the genera *Hyracotherium* and *Systemodon*, which are obviously near relatives, have been referred to distinct families on account of the presumption that while the one is the ancestral form of the horse tribe, the other has given origin to the tapirs.

Dr. Matthew, as already mentioned, transfers *Oligobunis*, which had been regarded as ancestral to *Spcothos*, to the Mustelidæ. The short muzzle lacks the constriction in front of the carnassials distinctive of the early Canidæ, and the other dental features,

especially the presence of a distinct notch between the blades of the lower carnassial, are of a musteline type. Very interesting is a short-jawed musteline described as *Megalictis*, with teeth intermediate between those of *Oligobunis* and the living *Gulo* and *Mellivora*, to which latter it appears nearly related.

Certain new rodents, some referable to the well-known beaver-like genus *Steneofiber*, and others to the allied *Euhapsis*, are also described by Dr. Matthew; but these may be passed over without further notice.

Of more general interest is *Epigaulus*, a new horned rodent from the Miocene of Kansas, described by Mr. J. W. Gidley in the *Proceedings* of the U.S. National Museum (vol. xxxii. p. 627). It is the most specialised member of the Mylagaulidæ yet discovered, with the large nasal horns placed just in front of the orbits. Mr. Gidley concludes that while this extinct group of specialised rodents has some remote affinities to the beavers (Castoridæ) and a nearer relationship to the squirrels (Sciuridæ) it has a greater number of characters in common with the sewellels (Haplodontidæ), and also shows certain resemblances in general proportions to some of the Asiatic mole-rats (Spalacidæ). The latter, however, seem due more to modification of the skeleton for burrowing than to real relationship. In tooth-development the Mylagaulidæ have surpassed in specialisation the other Sciuromorpha, and could not have been ancestral to any of the living forms.

Squirrel-like rodents of the genera *Paramys* and *Sciuravus*, from the Eocene deposits of the Wasatch and Wind Rivers, have been described during the year by Mr. F. B. Loomis (*Amer. Journ. Sci.* for February). Both genera are nearly related; the former, at any rate, exhibiting many squirrel-like characters, and being probably one of the ancestral types of the modern family.

The extermination of the urus, or aurochs—the ancient European wild ox (*Bos taurus primigenius*)—forms the subject of a very interesting paper by Dr. A. Mertens published in the *Abhandlungen und Berichte* of the Magdeburg Museum. The author confirms the opinion that the name aurochs belongs to this animal, although it has been misapplied to the bison. It is likewise certain that in the time of Herberstain (the middle of the sixteenth century) aurochs were living in Poland, and were seen alive by him; and, according to other testimony, a herd of thirty was living in the Jaktorowka (or Wiskitki)

forest of the Masovia district of Poland in the year 1564. By 1599 the number was reduced to twenty-four, while in 1602 only four remained, these being reduced in 1620 to one cow, which appears to have been killed seven years later. A few half-domesticated individuals were, however, living in captivity in 1627. Herberstain's statement that the aurochs was typically black, with a light dorsal streak, is accepted. Other evidence tends, however, to show that there were grey aurochs in Poland, and red ones in Central Germany. This is not the only paper on this subject published during the year, as Dr. M. Auerbach, in the *Verhandlungen der Naturwissenschaftlichen Vereins* of Karlsruhe (vol. xx.) has given descriptions of a number of remains of both the urus and the bison preserved in the museum of that city.

Little appears to have been contributed during the year to our knowledge of the past history of the deer tribe. A considerable collection of antlers of reindeer and elk from the Pleistocene of Olai, Livonia, has, however, been described by Mr. G. Schweder, in the *Korrespondenzblatt naturf. Vereins* of Riga, where it is pointed out that in 1831 the name of *Cervus gallinus* was suggested by Fischer for the fossil elk antlers from this district. If, therefore, the European elk be eventually split up into local races, this name will have to be considered.

In the article on vertebrate palæontology contributed to Science Progress a year ago, reference was made to Dr. Lönnberg's opinion as to the near relationship of the "Irish elk" to the reindeer, and to certain arguments which had been advanced against that view. These arguments have been endorsed and strengthened in a note contributed by Dr. R. F. Scharff to the May number of *The Irish Naturalist.*

If the work on fossil Cervidæ has been small and unimportant, that accomplished in America in connection with the extinct ruminating oreodonts, or merycoidodonts, and fossil swine has been extensive. Some of these merycoidodonts are described in the paper by Mr. Peterson already referred to; but by far the most interesting is one from the Miocene of Nebraska, for which Mr. E. Douglass (*Annals* of the Carnegie Museum, vol. iv. No. 2, 1907) proposes the name of *Pronomotherium.* The skull is characterised by its shortness, the presence of a small pair of horns above the orbits and of a large cavity in advance of the same, as well as by the extraordinary depth of the hind part

of the lower jaw. In the opinion of Mr. Douglass, the owner of this skull was furnished with a proboscis of considerable length. The development of horns is probably a sign that the genus belonged to a decadent, or over-specialised, member of the group. Another member of the same genus is described by the author in the *Bulletin* of the American Museum, vol. xxiii. art. 32. Skulls of a number of other forms of these curious "ruminating hogs" are described in the same two papers, but since they belong to well-known genera, further mention is needless.

The work on the swine group is suggestive of somewhat unnecessary genus-making. In a paper published in the *Memoirs* of the Carnegie Museum (vol. ii., No. 8) Mr. O. A. Peterson described a peccary from the Miocene of Nebraska under the name of *Thinohyus siouxensis*. According, however, to Dr. Matthew, *Thinohyus* is inseparable from the White River genus *Perchœrus*; but instead of referring Peterson's species to that genus, he proposes the new generic name *Desmathyus* for a more or less closely allied animal, and regards "*Thinohyus*" *siouxensis* as a primitive aberrant form transitional from *Perchœrus*. Surely in such a case it would be better to include all three forms in one genus. In these Miocene and Oligocene peccaries there were three pairs of upper incisor teeth, instead of the two of the modern *Dicotyles*; but in some of the Miocene species the outer pair is much reduced in size. These later Miocene forms also agree with modern peccaries in having the tympanic bulla of the skull filled with cancellous bony tissue, whereas in the typical *Perchœrus* it is empty.

The evolution of the horse and the mutual relationships of the different domesticated breeds are subjects which continue to attract the attention of investigators on both sides of the Atlantic. Dr. R. S. Lull has drawn up a useful summary of the main facts connected with the evolution of the horse family, which first appeared in the March number of *The American Journal of Science*, but has been reprinted in pamphlet form as a guide-book to the collection in the Peabody Museum at Yale University. Prof. J. C. Ewart, on the other hand, has devoted his attention to a large series of horse-skulls discovered in the Roman fort at Newstead, near Melrose, and published the results of his investigation in the *Transactions* of the Royal Society of Edinburgh, with a summary in an article in the

April number of *The Quarterly Review*. On the evidence of these skulls Mr. Ewart proposes to recognise three types of domesticated horse, severally distinguished by the degree of inclination of the face to the basal axis of the cranium.

During the year Dr. Ameghino has published in the *Anales* of the Buenos Aires Museum (vol. xvi.) a figure of the skeleton of *Hippidium*, a small extinct South American horse remarkable for the great length of the nasal bones and of the slits by which they are separated from the adjacent elements of the skull. A model of the skeleton from which Dr. Ameghino's figure is taken has been installed in the British Museum (Natural History).

A new rhinoceros from Portugal has been named by Mr. Roman in the paper cited above, and Mr. F. Toula (*Jahrb. Geol. Reichsanstalt*, vol. lxvi.) records a new variety of *Rhinoceros mercki* from Austria.

Brief mention may here be made of another paper by Dr. Ameghino in the serial just quoted, where it is pointed out that the skulls of some of the South American toxodonts are furnished with rudimentary horn-like prominences.

As regards the proboscidean group, reference has already been made to the discovery of an extinct elephant and mastodon in South Africa; and it should be added that Dr. Schlosser, in a paper quoted above, has recorded remains of *Mastodon longirostris* from the upper Tertiary of Spain. To the energy of Miss Dorothea Bate science is indebted for the discovery of remains of a pigmy elephant (*Elephas cretensis*) in the caves of Crete; these having been described and figured by their discoverer in the August issue of the Zoological Society's *Proceedings*. To the exhibition galleries of the American Museum has been added a complete skeleton of the Columbian mammoth, discovered in Indiana in 1903. According to a preliminary description by Prof. H. F. Osborn published in the *Bulletin* of the American Museum (vol. xxiii. p. 255), the mounted skeleton stands 10 ft. 6 in. at the shoulder. The largest known tusk of this species or race of mammoth appears to be one referred to in *Science* for 1907 (vol. xxv. p. 971) by Mr. C. H. Sternberg, of which the exposed portion measured 14 ft. in length, with a basal diameter of 8 in.

Of the few papers on fossil cetaceans which have come under the writer's notice, one by Mr. F. W. True, issued as No. 1694 of the quarto series of publications of the

Smithsonian Institution, is devoted to an imperfect skull obtained in 1847 from the Eocene marl of the Ashley River, South Carolina. In the year of its discovery appeared a brief account of the specimen by Mr. M. Tuomey, who referred it to *Zeuglodon*; and two years later it was named *Z. pygmæus* by Prof. J. Müller. The specimen afterwards came under the notice of Prof. L. Agassiz, who caused a plate to be prepared, with the lettering *Phocodon holmesi*; but for some reason this plate was never issued, and in 1895 Prof. E. D. Cope referred the specimen to a new genus, under the name of *Agorophius pygmæus*. Despite the fact that when first figured the specimen had a single *Squalodon*-like tooth in the maxilla, a suggestion was made that the species might be an ancestral rorqual. In Mr. True's opinion, *Agorophius* is, however, a squalodont, differing in cranial characters from *Squalodon* itself. The European *Squalodon ehrlichii* has a broad rostrum recalling the Carolina genus, and may be a connecting link between *Squalodon* and *Agorophius*. The type specimen of another American Tertiary cetacean—*Anoplonassa forcipata*—has also been redescribed by Mr. True (*Bull. Mus. Zool. Harvard Coll.*, vol. li. p. 97), who regards it as representing a member of the Physeteridæ allied to the modern *Mesoplodon*. The rarity of cetacean remains in the Eocene clay of Barton, Hampshire, fully justifies Dr. C. W. Andreas in describing (*Quart. Journ. Geol. Soc.*, vol. lxiii. p. 124) a vertebra of *Zeuglodon wanklyni*; this being the third known specimen of that species. Of the two earlier ones, the second was incorrectly referred to a rorqual, under the name of *Balænoptera juddi*. Of greater importance is a memoir by Dr. O. Abel, published in the *Denkschriften* of the Vienna Academy (vol. lxxxvi.), on the rudimentary pelvis of cetaceans. At the conclusion of this memoir the author points out that the development of a caudal fin in cetaceans and sirenians has rendered a functional pelvis superfluous in both groups. Among cetaceans and dugongs the reduction has followed parallel lines, culminating in a retention of portions of the ilium and ischium. On the other hand, the manatis have followed a line of their own, and have retained only the ischium.

The only important work on fossil marsupials which has come under the writer's notice is an attempt by Dr. E. C. Stirling (*Nature*, vol. lxxvi. p. 543) to restore the external

form of *Diprotodon*. As regards general contour, Dr. Stirling has followed the lines of a wombat, although with a certain increase in the relative length of the limbs. Since, however, the teeth and fore part of the skull are of the kangaroo type, with some indications of a greater development of the soft parts of the nasal region, the peculiar puffy and cushion-like muzzle of the kangaroo has been introduced, in a somewhat accentuated degree, into the restoration.

Dr. Broom's recent work on the ancestry of mammals receives mention at the end of the section on reptiles.

Work on fossil birds during the period under review appears, as usual, to have been but small, The discovery of a leg-bone of a heron-like species in the Eocene of Egypt has been already mentioned. Another lower Tertiary bird has been identified by Dr. O. Abel (*Centralblatt für Mineralogie*, 1907, p. 450) in the shape of a large coracoid from the upper Eocene of Choctaw County, Alabama, which had been mistaken for a zeuglodont bone. That this bird, for which the name *Alabamornis gigantea* has been suggested, was of huge size, and therefore flightless, is certain; but whether it was related to *Diatryma* of the North American and *Gastornis* and *Remiornis* of the European Eocene must for the present remain doubtful.

In connection with birds, reference may be made to an exceedingly able and interesting paper on the origin of flight in vertebrates by Baron Francis Nopcsa, published in the June issue of the *Proceedings* of the Zoological Society of London. In the opinion of the author, bats and pterodactyles are the descendants of four-limbed arboreal creatures in which, owing to the development of a flying-membrane (*patagium*), the fore-legs had become to a certain degree adapted for flight, and in consequence less well suited for terrestrial progression. Birds, on the other hand, are regarded as having been evolved from running bipedal dinosaur-like reptiles, in which the front limbs, owing to a flapping movement, became modified into wings, without any concomitant loss of the cursorial power of the hind-limbs. It is on account of this superior mode of development that birds have outstripped both pterodactyles and bats in the contest for the "command of the air." They may be likened to "äeroplanes," whereas their rivals have not passed the "air-ship" stage. Baron Nopcsa's paper is illustrated with a restoration of a cursorial "pröavis."

From the preceding paragraph we are led by an easy transition to pterodactyles, on which group, as represented in the upper Jurassic strata of Swabia, Dr. Felix Plieninger has contributed an important memoir to the Stuttgart *Palæontographica* (vol. liii., Nos. 4–6). Several beautifully preserved skeletons from the Lithographic Stone of Nusplingen and other Swabian localities are figured, for one of which the new name *Rhamphorhynchus kokeni* is proposed. The generic position of the English *Scaphirhynchus purdoni* is also discussed at some length.

Having completed the survey of the extra-European Triassic dinosaurs, Prof. F. von Huene has commenced in *Geologische und Palæontologische Abhandlungen* (Jena, 1907) a detailed description of their European representatives. The first part is devoted to the dinosaurian remains from the Keuper of the Nürenberg district, first brought to notice by Engelhardt in 1834, and afterwards referred to *Thecodontosaurus* and *Palæosaurus* of Riley and Stuchbury, which were long regarded as a general receptacle for Triassic dinosaurs of all descriptions. Dr. von Huene refers all the remains described in this part to von Meyer's genus *Plateosaurus*, of which, in addition to the typical *Pl. engelhardti* and *Pl. queustedti*, he recognises the new species *Pl. reinigeri* and *Pl. erlenbergiensis.*

A redescription of the type skeleton of the sauropod dinosaur *Morosaurus agilis* forms the subject of a paper by Mr. C. W. Gilmore in the *Proceedings* of the U.S. National Museum (vol. xxxii. p. 151), in which it is suggested, on account of the primitive character of the vertebral column, that this species may belong to the genus *Hoplocanthosaurus.* To the sauropod genus *Pelorosaurus*, typified by a humerus from the English Wealden, Mr. H. E. Sauvage (*Bull. Soc. Boulogne-sur-mer*, vol. vii.) refers part of a femur from the Jurassic of the Boulonnais; also mentioning that the teeth from the same deposit described as *Neosodon* and *Caulodon* likewise pertain to the same genus.

Dinosaurian remains from the Jurassic and (?) Cretaceous of Madagascar have been described by Mr. A. Thevenin in the *Annales de Paléontologie*, vol. ii. pt. 3. Some of these are referred to *Bothriospondylus madagascariensis*, a species described several years ago by the present writer; while others are assigned to *Titanosaurus* and *Megalosaurus.* As *Titanosaurus* is typically

from the Cretaceous of Central India, its reappearance in Madagascar has an important bearing on the theory of a former land-connection between those two countries.

Great interest also attaches to a couple of skeletons of a small reptile from the Trias of Lossiemouth, Elginshire, regarded by Dr. H. Smith Woodward (*Quart. Journ. Geol. Soc.*, vol. lxiii. p. 140) as representing a new type of dinosaur, for which the name *Scleromochlus taylori* is proposed. The pelvis is unlike that of the Rhynchocephalia, while the absence of bony armour and the apparent perforation of the acetabular cavity of the pelvis distinguish the skeleton from the Parasuchia. On the other hand, the observable characteristics seem distinctly dinosaurian, and appear to indicate relationship with the Triassic American genera *Ammosaurus*, *Anchisaurus*, and *Hallopus*. The large skull, apparently toothless jaws, and the firm union of four elongated metatarsal bones are, however, peculiarities of the Elgin reptile, which was evidently a specialised type. The extreme lightness of the skeleton, coupled with the elongation of the hind-limbs, suggests leaping habits.

Passing on to the Parasuchia, or Phytosauria, we find that in America Mr. J. H. Lees has described in *The Journal of Geology* (vol. xv. p. 121) a new genus and species from the Trias of Wyoming, under the name of *Palæorhinus bransoni*. The genus is akin to the American *Rhytidodon*, from which it appears to be mainly distinguished by a difference in the form of the ilium. In Europe Prof. E. Fraas (*Jahreshefte Ver. Naturkunde Württemberg*, 1907, p. 101) has made known a new species of *Aëtosaurus* from the upper Stubensandstein of Stromberg, near Pfaffenhofen, under the name of *A. cristicauda*. In regard to the group to which these reptiles belong, it is remarked that one of the most important differences from the typical Parasuchia is the smaller share taken by the ischium and pubis in the formation of the acetabulum. This and other differences do not, however, necessitate ordinal separation from that group, of which the Aëtosauria may be regarded as a specialised branch, distinguished by the peculiar type of the dermal armour.

The acquisition by the Belgian Museum of a number of vertebrate remains from the Eocene strata of Erquelinnes, near Hainault, has enabled Dr. L. Dollo (*Bull. Soc. Belge Géol.*, vol. xxi. p. 3) to describe a new type of crocodile, to which he has given the name *Eosuchus lerichei*. Although a member

of the modern group of the order, this genus, in the less backward position of the posterior nostrils, makes some approximation to Jurassic forms.

A memoir on the structure and adaptive modifications of the armourless marine Jurassic crocodiles of the genus *Metriorhynchus*, by Mr. G. von Arthauber (*Beitr. Pal. Ost.-Ung.*, vol. xix. p. 287, 1906) has given rise to a considerable amount of discussion. In the *Centralblatt für Mineralogie* (1907, p. 225) Dr. O. Abel criticised the memoir somewhat severely, and published an amended restoration of the external form of *M. jækeli*, in which the reptile is represented with flipper-like limbs and a double-lobed tail-fin. Later on in the same volume (p. 353) further criticisms are published by Mr. E. Auer. To all these Mr. von Arthauber replied in the *Centralblatt* (p. 385), where his own views are maintained; while in a second paper he gave further details with regard to the structure of the skeleton of the hind-limb, directing special attention to the arrangement of the tarsal bones. Another contribution to the osteology of the genus, as represented by *M. superciliosus* of the English Oxfordian, has been made by Mr. E. T. Leeds in *The Geological Magazine* (decade 5, vol. iv. p. 314), where attention is directed to the confusion which has arisen owing to a portion of the coracoid having been wrongly described as the scapula.

Among more typical crocodiles, Mr. R. W. Hooley (*Quart. Journ. Geol. Soc.*, vol. lxiii. p. 50) has described a fine skull and other remains of *Goniopholis crassidens*, obtained after the fall of the cliff of Wealden strata at Atherfield, Isle of Wight. The identification by Dr. A. S. Woodward (*l.c.*, p. 132) of the same genus, in the form of *G. Hartti*, in the Cretaceous rocks of Bahia, Brazil, is of considerable interest.

In the only paper on fossil snakes which has come under the writer's notice, Dr. W. Janensch (*Archiv. für Biontologie*, vol. i. p. 313) discusses the monster serpent *Pterosphenus schweinfurthi*, of the Fayum Eocene, and supports the view that it is rightly associated with the European *Palæophis* in a separate family, the Palæophidæ, which is more nearly related to the pythons than to the sea-snakes, despite the fact that its representatives were marine.

Important information with regard to the osteology of the ichthyosaurs has been furnished by Dr. C. W. Andrews in a paper on the Oxfordian *Opthalmosaurus leedsi* published in the May number of *The Geological Magazine*. This species is

remarkable for the great variation exhibited by the bones of different individuals, this feature apparently indicating that these reptiles were in a state of developmental inæquilibrium. A restoration of the back of the skull has been attempted, which shows that the whole arrangement is adapted to give the greatest possible rigidity to the quadrate—doubtless for the purpose of affording a firm support for the long jaws. The stapes, which, according to the author, has lost its function as a bone of the internal ear, is firmly united at one end with the basi-occipital, whilst its other extremity is fitted into a pit-like facet of the quadrate.

This conclusion as to the stapes (or columella) having no connection with the auditory function is disputed by Dr. Louis Dollo in a note published in the *Bulletin de la Société Belge de Géologie*, vol. xxi. p. 157, who suggests that it has a kind of secondary auditory function, connected with the diving or "sounding" habits of the ichthyosaurs. Such a function he considers to be connected with a modification of the cerebral circulation and paralleled by the conditions obtaining in cetaceans.

The description by Mr. C. W. Gilmore, in the March number of *The American Journal of Science* (vol. xxiii. p. 193), of a new species of *Baptanodon* from the Jurassic of Wyoming is of interest from the fact that the remains are less incomplete than those of any other known specimen of the genus.

On account of the excessive width of the front paddle, which comprises eight longitudinal rows of bones, the skeleton of a new species of ichthyosaur (*Ichthyosaurus platydactylus*) from the Chalk of Kastendamm, Hanover, described by Mr. F. Broili in vol. liv. of the Stuttgart *Palæontographica*, is of more than ordinary interest. At the close of his memoir the author illustrates diagrammatically the fact that in the Liassic ichthyosaurs the front paddles and tail-fin are relatively much larger than in the Cretaceous species, this apparent loss in the latter of propelling power being compensated by an increase in the width of the fore-paddles.

Considerable importance attaches to an account of the Cretaceous pliosaurian genus *Brachauchenius* given by Mr. S. W. Williston in No. 1540 (vol. xxxii. p. 477) of the *Proceedings* of the United States National Museum. *Brachauchenius*, which is known from West Kansas, is regarded by its describer as closely related to *Pliosaurus*, from which it differs in having single-

headed cervical ribs. From other plesiosaurians in which the skull is fully known, it differs in that the palatines meet each other in the middle line, but this is a feature which the author thinks will be met with in *Pliosaurus*. The assumed relationship of the plesiosaurs to chelonians is disputed. The latter lack, for instance, epiphyses to the humerus, while such resemblance as exists between the shoulder-girdle in the two groups is due to adaptation. Chelonians are without the parietal foramen of plesiosaurs, and retain the hypocentral mode of articulation of the ribs, whereas in plesiosaurs the ribs are attached to the transverse processes of the vertebræ. Sauropterygia are, indeed, probably descended from theriodont ancestors, while Chelonia appear to be derived from a cotylosaurian type, both being widely sundered from ichthyosaurs and rhynchocephalians.

Brief reference will suffice to a paper by Mr. F. Broili, published in the *Centralblatt für Mineralogie*, on certain reptilian remains from the Kössen beds of the Trias of the Alps. From the form of the dorsal vertebræ and the stoutness of the abdominal ribs, the specimens evidently indicate one of the nothosaurs, or primitive sauropterygians. Their large size suggests affinity with *Partanosaurus*, but in structure the vertebræ appear to display characters intermediate between those of that genus and those of *Nothosaurus*.

In a communication to *Science* for 1906 (vol. xxiv. p. 184) Prof H. F. Osborn states that the American Museum has received from the Brazilian Coal Commission natural casts of a small Permian reptile near akin to *Stercosternum* of the same country and horizon, and to *Mesosaurus* of the South African Permian. An opinion is quoted to the effect that none of these reptiles are related to the plesiosaurs (Sauropterygia).

The huge Eocene turtle known as *Eosphargis gigas*, typically from the London Clay, also occurs in the corresponding formation of Belgium, where an almost complete specimen has been discovered at Quenast. According to Mr. Dollo (in the serial already quoted) the upper shell, or carapace, of this turtle is quite different from that of the modern leathery turtle, and is of the general type obtaining in ordinary turtles.

Remains of soft river tortoises (Trionychidæ) from various North American horizons form the subject of a paper by Mr. O. P. Hay in the *Bulletin* of the American Museum, vol. xxiii. art. 34.

The list of rhynchocephalian reptiles has been increased by Dr. R. Broom's description (*Trans. S. African Phil. Soc.*, vol. xvi. p. 379) of the new genus *Palacrodon browni*, from the Triassic division of the Karoo series of South Africa. Dr. Broom, it may be added, restricts the Rhynchocephalia to reptiles with a radiating pelvis, and therefore excludes the Indo-European Triassic genera *Palæohatteria*, *Rhynchosaurus*, and *Hyperodapedon*, in which the pelvis is plate-like. On this view, *Palacrodon* is the only Triassic representative of the order.

During the year important work has been accomplished in that remarkable group of Permian reptiles known as the Pelycosauria. In an article published in the *Bulletin* of the American Museum (vol. xxiii. p. 265), Prof. H. F. Osborn has given a figure of the skeleton of the Texan "ship-lizard," *Naosaurus claviger*, with an attempted restoration of the creature's bodily form. The length of the skeleton is 6½ ft. Among the more striking features are the enormous head, armed with powerful teeth, and the great height of the spines of the vertebræ of the long back, which are furnished with transverse bars recalling yard-arms, and thus suggesting the name of the genus. The restoration of the allied genus *Dimetrodon* has been essayed by Dr. E. C. Case in vol. xv. of *The Journal of Geology*.

Of far more importance is the monographic revision of the Pelycosauria by the last-named naturalist, published by the Carnegie Institution of Washington. After discussing the various opinions as to the proper systematic position of these reptiles, the author concludes that they really form a subordinal group of the Rhynchocephalia. They all had notochordal vertebræ, with intercentra, and two-headed ribs; but it was only in the more specialised forms that the spines of the vertebræ attained the characteristic development. All were carnivorous. The more primitive representatives of the group, constituting the new family Poliosauridæ, are not far removed from the Permian rhynchocephalians of the suborder Protorosauria; but the other forms display great specialisation, which to a great extent obscures their rhynchocephalian affinities. The great development of the dorsal spines of the vertebræ is the most obvious feature of this specialisation, but it is accompanied by others such as the roofing over of the upper temporal fossa, the mode of rib-attachment, and the enormous size of the claws. Although in earlier stages the spines may have served some useful purpose,

in their full development they are regarded by Dr. Case as a mere redundancy of growth, possibly an example of that development of spiny outgrowths and excrescences which appears to mark a decadent group. In addition to North America, pelycosaurs occur in Bohemia (*Naosaurus*), and not improbably in Central Germany and France; but they appear to be unknown in Russia and Africa.

Coming to the anomodont, or theromorphous, group, we find Dr. R. Broom, in the journal already quoted (p. 376), describing a new species of the theriodont genus *Ælurosaurus* from Aliwal North; while in the *Annals* of the Natal Museum (vol. i. p. 168) he makes known a new *Dicynodon* and a new *Scymnosaurus* from that colony. *Scymnosaurus*, which is one of the carnivorous types, appears to be nearly related to *Inostrancevia*, of the Russian Permian. In America, Dr. Case has recently described two nearly complete skulls of the cotylosaurian genus *Bolosaurus*, from the Permian of Texas; the paper forming article 28 of vol. xxxii. of the *Bulletin* of the American Museum.

The relationship of the anomodonts (in the widest sense of that term) to mammals has continued to attract the earnest attention of Dr. Broom, who, in a paper published in the *British and South African Association's Report* for 1907 (vol. iii. p. 12), summarises the evidence in the following words:

"We may regard it as fairly well established that mammals are descended from one of the mammal-like reptiles, and almost certainly from a cynodont (theriodont). The known cynodonts are mostly too large to have been the ancestors, but there is evidence of there having been numbers of small forms, and it was probably from one of these that the new stock arose. And I think we may also safely conclude that all mammals are descended from one common ancestor. By one or two authorities it has been argued that the monotremes are a race apart, and it has even been suggested to remove them from the mammals altogether; but all recent research has confirmed the earlier view that monotremes are typical mammals, and in skeletogenesis are quite as near the marsupials as the marsupials are to the higher forms."

It may be added that Dr. Broom is firmly convinced that *Tritylodon* is a mammal. "As it can be shown," he writes, "from the structure of the molars, that the lower jaw was moved backwards and forwards, as in rodents, it is impossible

that the articulation can have been of the theriodont type, and we must conclude that Owen's opinion was correct that *Tritylodon* was a mammal."

The following interesting case of apparent affinity between anomodonts and mammals given by Dr. Broom in the *Zool. Anzeiger* (vol. xxviii. p. 106) may likewise be quoted. It has long been known that in the human subject, and apparently in mammals generally, there exists a remarkable peculiarity in the development of the metacarpal bones of the hand and the metatarsals (or corresponding elements) of the foot. In the metacarpal (or first joint) of the thumb and the metatarsal of the great toe the separate cap, or epiphysis, is situated at the basal extremity, whereas in the other metacarpals and metatarsals it is developed at the opposite end of the bone. The solution of the puzzle has been found by Dr. Broom, who has discovered that in anomodonts the outermost of the second row of bones in the wrist and the ankle is so elongated as almost to resemble a metacarpal or metatarsal. Owing to this elongation, the main joint of the thumb and great toe appears to have been between the wrist or ankle and the metacarpal or metatarsal (as the case may be), while in the other digits this joint was situated between the metacarpal or metatarsal and the first segment of the fingers or toes. Those ends of the bones which took part in the main joint would be the last to ossify, and would determine the points where the caps, or epiphyses, would form. The anomaly in this respect in the human subject seems therefore to be a direct inheritance from reptilian ancestors.

Work on labyrinthodonts (stegcephalians) appears to have been limited. Prof. H. G. Seeley has, however, described in the October number of *The Geological Magazine* a new genus and species from the Karoo series of South Africa under the name of *Ptychosphenodon browni*; and Mr. E. C. Case (*Bull. Amer. Mus. Nat. Hist.*, vol. xxiii. p. 665) has published notes on remains from the Permian of Texas which throw additional light on the structure of the genus *Zatrachys*.

Perhaps the most important contribution to fossil ichthyology is a memoir by Mr. C. R. Eastman on the Devonian fishes of the New York district, published by the New York State Museum. The memoir, in addition to a large mass of descriptive and technical detail, is written from a broad and philosophical

standpoint, and contains much information and many suggestions as to the habits, phylogeny, and migrations of these Palæozoic fishes. An interesting generalisation, based upon examples from numerous groups, is that those types of fishes which show no tendency to progressive evolution undergo a kind of degenerative development so as to assume a more or less distinctly eel-like form of body. This is true among the acanthodian sharks, which declined towards the close of the Palæozoic epoch, dying out with the Permian. Their fins, being essentially skin-structures, were incapable of strengthening by bony rods, after the fashion obtaining in other contemporary and competing groups. Hence these fishes, as they became worsted in the struggle for existence, changed their habits and their physical structure. Devonian species, for example, with the graceful fusiform proportions of swift swimmers, became replaced in the Carboniferous by overgrown, unwieldy creatures like *Gyracanthodes*, which were evidently grovellers in the slimy mud of land-locked seas and estuaries; while the expiring Permian forms exhibit a distinct approximation to the eel-like type of body.

Passing southward of the isthmus of Panama, fish-remains from the Cretaceous of Bahia form the subject of a paper by Dr. A. Smith Woodward in *The Quarterly Journal of the Geological Society* (vol. lxiii. p. 131). These indicate a curious mixture of Wealden and middle or upper Cretaceous types; *Lepidotus mawsoni* being, for instance, near akin to the well-known *L. mantelli* of the English Wealden, while *Megalurus mawsoni* appears to be a Jurassic rather than a Cretaceous form of amioid fish. On the other hand, the freshwater herring *Diplomystus longicostatus* is a member of a genus common in the upper Cretaceous of the Lebanon and the lower Tertiaries of Europe and North America. The new genus and species *Mawsonia gigas*, a member of the cœlacanth ganoids, may be either Jurassic or Cretaceous, although more probably the latter.

The same author has contributed to *The Geological Magazine* (decade 5, vol. iv. p. 193) an account of the fossil fish-fauna of Pernambuco, in which the new species *Rhinoptera prisca* is described.

From the Lahontan beds of Nevada have been obtained remains of a stickleback, which is regarded by Mr. O. P. Hay

(*Proc. U.S. Nat. Mus.*, vol. xxxii. p. 271) as a subspecies of the existing freshwater *Gasterosteus williamsoni*, whose range extends from Alaska to California. The interest of the identification lies in the inference that the Lahontan beds are not older than the Pleistocene.

Pycnodont fishes from the Cretaceous of the Lebanon form the subject of a paper by Mr. E. Henmy in the *Centralblatt für Mineralogie* (1907, p. 360); while Mr. Erich Heineke has published in vol. viii. pt. 3 of *Geol. und Pal. Abhandlungen* a detailed account of the ganoids and teleosteans of the Lithographic Limestone of Nusplingen.

Of *Amyzon brevipinne*, a fish hitherto known only by a single specimen from the (probably Oligocene) Amyzon beds of British Columbia, Mr. L. M. Lambe has described, in the *Transactions* of the Royal Society of Canada (vol. xii. p. 151), a second and less incomplete example from the same horizon. The palæoniscid *Myriolepis hibernica*, from the Irish Coal Measures, forms the subject of a note in the *Annals and Magazine of Natural History* for December 1906 (ser. 7, vol. xviii. p. 416).

In the elasmobranch group Prof. E. Fraas (*Neues Jahrb, Mineral.*, 1907, vol. i. p. 1) gives further details of the structure of the rostrum and dentition of *Propristis schweinfurthi*, a sawfish from the Egyptian upper Eocene. In this fish the "saw," which is slender and abruptly truncated at the tip, and attained a length of rather more than two metres, has the same internal structure as in *Pristis*, but carries teeth only in its terminal fourth; these teeth being short and broad-crowned, with sharp fore-and-aft edges, which slightly overlap one another, thus forming a sawing implement of great power and efficiency. Of the well-known selachian genus *Hybodus*, Prof. E. Koken gives further structural details in a memoir of considerable morphological importance published in *Geol. und Pal. Abhandlungen*, vol. ix. pt. 4.

The controversy as to the mutual relationships and general affinities of the more primitive Palæozoic fishes and fish-like creatures is maintained with unabated vigour in several papers by Dr. O. Jaekel and Dr. L. Dollo. The first group of these papers, most of which were published in 1906 (although they were not noticed in the article on the present subject in last year's SCIENCE PROGRESS), deal with the imperfectly known

ptyctodonts, as represented by the genera *Ptyctodus*, *Rhynchodus*, *Palæomylus*, and *Rhamphodus*. In the British Museum *Catalogue of Fossil Fishes* the group, of which only three genera were then known, is placed in the chimæroid order of the Holocephali. In a paper on Devonian fishes published in *Sitz.-Ber. Ges. nat. Freunde, Berlin*, for 1906 (p. 77) Dr. Jaekel accepted this arrangement, with certain modifications. Somewhat later Dr. Dollo (*Bull. Soc. Belge Géol.*, vol. xx. p. 133) denied that the ptyctodonts were Holocephali, although not giving his full reasons for the statement. This was in June, 1906, and a month later Dr. Jaekel (*op. cit.*, p. 180) changed his previous opinion, and classed these fishes in the sturgeon group (Chondrostei). During the present year Dr. Dollo has published a paper (*op. cit.*, vol. xxi. p. 1) in which he not only maintains the opinion that the ptyctodonts cannot be classed with the chimæroids, but goes a step further, and definitely places them in the Arthrodira, that is to say with *Coccosteus* and its relatives. He also expresses his opinion that the Holocephali are specialised derivatives from the Palæozoic cochliodont sharks.

In another paper, on the new genus *Pholidosteus* and the mouth-opening and bodily form of the Placodermata, published in the *Sitz.-Ber. Ges. nat. Freunde, Berlin*, 1907 (p. 170), Dr. Jaekel reiterates the views expressed by him in 1902 in regard to this group. Our readers may be reminded that under the title of "Placodermata" the author unites the Ostracodermata (*Pteraspis*, etc.) with the Arthrodira (*Coccosteus*, etc.), although by most authorities the two latter are widely separated. Dr. Jaekel regards all the placoderms as true fishes, which, apart from the retention of certain ancestral features derived from amphibians, are related to the Holocephali (chimæroids), Elasmognathi (sharks and rays), and Chondrostei (sturgeons), with some indications of kinship to the Dipnoi (lung-fishes). These groups he would brigade together under the title of Hypostomata, in contradistinction to the Teleostomata, which include all other fishes.

The list of these primitive fishes has been augmented by Mr. E. Hennig's description, in the *Centralblatt für Mineralogie*, 1907, p. 584, of a new species from the Palæozoic rocks of Gerolstein, referred to the genus *Macropetalichthys* (an ally of *Asterolepis*), with the name *M. pelmensis*.

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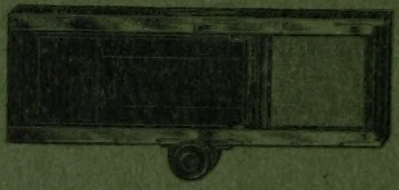

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